THE CRISIS IN SPACE AND EARTH SCIENCE

A TIME FOR A NEW COMMITMENT

A REPORT BY THE SPACE AND EARTH SCIENCE ADVISORY COMMITTEE

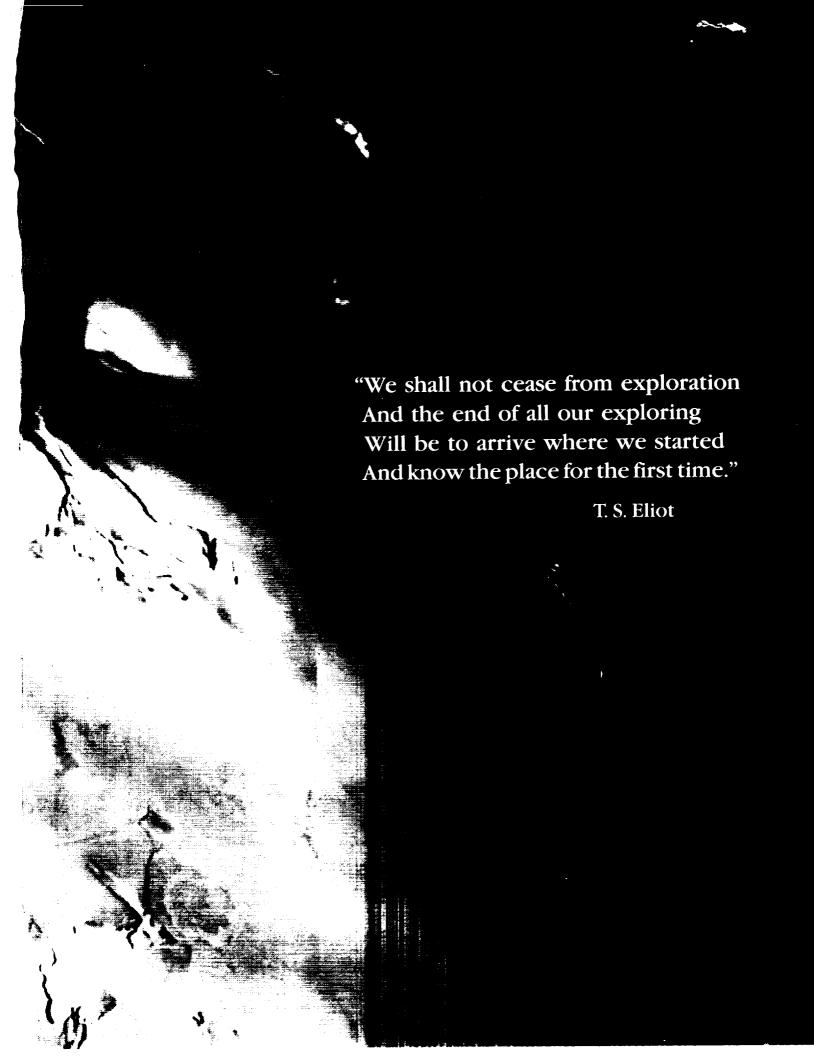
NASA ADVISORY COUNCIL

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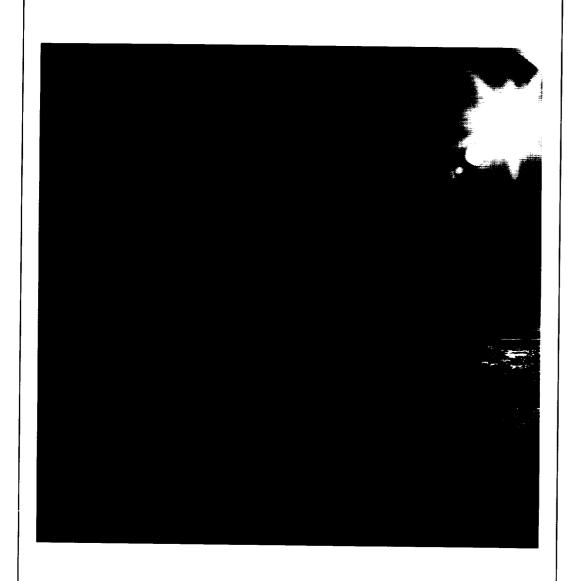
Foreword

Even before the tragic Challenger accident and the resulting hiatus in the space program of the United States, the space research community was experiencing a growing sense of uncertainty about its future. Outstanding scientific successes in the Space and Earth Sciences had opened new horizons of knowledge and stimulated numerous new scientific questions requiring observations and experiments in space. At the same time, the perceived scientific imperatives were beginning to outpace the funding resources and flight opportunities available, leading to stresses in the program. Questions concerning priorities were being raised without a clear vision of how to arrive at prioritizations both within and among scientific disciplines.

Such perceptions led the Space and Earth Science Advisory Committee (SESAC) of the NASA Advisory Council to begin two years ago a broad examination of the programmatic issues facing the U.S. Space and Earth Science Program. The intent was to examine in some detail the forces acting on the U.S. space research community and to make recommendations, insofar as possible, which would foster a scientifically productive program. A process was developed to assess priorities on the basis of both the scientific merits as well as the interrelated external factors which in reality must shape our nation's choices. The loss of Challenger and its valiant crew in January of this year has made this assessment by SESAC even more necessary and timely.

This report is the result of SESAC's deliberations. The NASA Advisory Council, with the issuance of the report, hopes and expects that the report will make a significant contribution toward defining the future direction of Space and Earth Science research in NASA and will help to ensure that the program will be a continuing source of pride for the American people. The Council believes that future advances in understanding of the Earth, the solar system, and the universe will be outstanding contributions by our civilization to the enlightenment and future of humanity.

Daniel J. Fink Chairman NASA Advisory Council



Overview

Results from the Space and Earth Sciences, in the last quarter century, have stimulated a profound curiosity about our universe and an awareness of our own planet. The astounding successes of science missions in space, ranging from weather satellites, to astronomical observatories, to planetary reconnaisance and surface sampling, have created a new sense of the wonder and unity of our environment and have produced an almost dazzling array of compelling new scientific questions yet to be answered. Science in space is an unparalleled intellectual adventure, a technological endeavor, and the necessary precursor to the next great journey of mankind envisioned by the National Commission on Space (1986).

The nation has also had an important emotional, as well as intellectual, investment in its successes in space and in space research. American leadership in penetrating the unknowns of our planet and solar system, and in unravelling the mysteries of the universe, is as important to the general public as to those directly engaged in the scientific endeavor. But preservation of a leadership position at the frontiers of science is precarious and can be maintained only through diligence and commitment; American preeminence is now in question. This report assesses the current health of the Space and Earth Science Program and identifies the requirements for a renewed commitment to excellence. It concludes that the program is facing grave difficulties and that specific steps must be taken to ensure its vitality and long-term future.

The Crisis in Space and Earth Science

Even before the Challenger accident, and the resulting hiatus in the space program, it was becoming clear that the nature of the Space and Earth Science Program was changing and that major stresses were developing as a result of those changes. Within the scientific community there was a growing sense of unease and frustration over the program's diminishing pace. As the result of a number of trends, it appeared that a major transition was taking place in the nature of the Space and Earth Science Program, but it seemed that this transition was occurring more by accident than as a matter of conscious policy. Decisions were being made that had long-term consequences on ways the program would be conducted, but the consequences of those decisions were largely unexamined. More and more missions were being identified as candidates for "New Starts" at a time when prospects for New Starts were becoming uncertain. The competition among prospective missions had escalated to a counterproductive level; there was a growing sense that the future vitality of whole fields of research depended on single decisions. The emergence of the Space Station as a major NASA initiative was raising questions as to whether NASA's science program would be reoriented around this facility. Questions concerning priorities were being raised without there being any obvious way to systematically address those questions. More and more scientific groups seemed to be competing for fewer and fewer flight opportunities. At the same time, the pressure to start major new missions seemed to be leading to an erosion of those smaller-scale, less glamorous, less visible activities that, in

many ways, formed the foundation of the program and ensured that the scientific return from major missions really justifies the investment.

Other grave difficulties were also appearing in the implementation of the program. Delays and cost overruns drained away resources that could have supported additional major missions or other important research; projects cancelled or repeatedly deferred after scientists had responded to Announcements of Opportunity, wasted the efforts of talented individuals; dependence on the Shuttle as the single launch vehicle introduced human safety as a crucial consideration into the program even for those missions where less risky alternatives should have been available; erratic funding patterns and continually shifting priorities created uncertainty for all components of the space research community. In view of the uncertain future and the lengthening time scales for execution of programs, talented individuals began to seek other opportunities.

All of these difficulties were dramatically amplified by the Challenger accident and the subsequent turmoil in the U.S. space program. Scientific spacecraft ready for launch were grounded. Delays of two years or more are inevitable; maintaining scientific teams and spacecraft readiness until missions are launched and results are available will be both difficult and costly. The mixture of launch capabilities available for future programs is not yet clear and there is a fear that the costs of the replacement Orbiter could threaten other elements of the NASA program.

As a result of many of these perceptions, two years ago the NASA Space and Earth Science Advisory Committee (SESAC) embarked on a wide-ranging examination of the programmatic issues facing the U.S. Space and Earth Science Program. The intent of this study was to determine the nature of changes underway, to understand the implications of those changes, and to make recommendations to enable NASA to proceed with a long-term, productive program in the Space and Earth Sciences. A major goal of the work was to develop a more rational process for making decisions, especially decisions concerning major new initiatives. The fundamental task of this effort was to determine how to optimize the use of the limited available resources in such a way as to construct the best possible scientific program.

All of these concerns became even more urgent in the wake of the Challenger tragedy.

Given the current critical circumstances and the clear threats to the vitality of the future program, careful examination of the premises upon which the NASA Space and Earth Science Program is based, planned, and executed is clearly in order. This report proceeds from such a fundamental examination to a series of recommendations intended to guide the conduct of the program in the years ahead.

Vitality in Science

Before arriving at any conclusions, we must first address what is required to ensure the vitality of NASA's program. Scientific vitality comprises many elements. They include:

- Stimulating questions. The success of the Space and Earth Science Program can be traced in part to the abundance of stimulating questions about our environment and place in the universe. Although some disciplines of space research are more mature than others, stimulating questions abound in all the disciplines.
- Observations and experiments. There must be a steady flow of experiments and observations, discovery, and reconnaissance. Scientists first search for new phenomena or for new ways of viewing known phenomena. Once a discovery is made, reconnaissance, systematic observation, and analysis begin with the goal of acquiring more complete understanding.
- Theory and models. Comprehensive theories and useful models spring almost naturally from a carefully planned base of observations. Observations validate theoretical predications or lead to creation of new theories, which, in turn, must be judged in terms of additional data.
- Talented and dedicated people. Essential to scientific progress is the involvement of talented, dedicated people driven to satisfy their curiosity about nature. They acquire a command of existing knowledge in order to make new contributions. Dedication alone, however, is not enough. Aspiring young scientists must have the support of a strong educational system in which they can learn by working with established researchers on substantive scientific questions.
- A perceived future. Any healthy science must have goals and opportunities that are perceived to be exciting and important both by specialists in the field and by the public at large. There must also be favorable prospects for the continuing support of those endeavors in order to drive the development of new levels of technological sophistication and scientific understanding.

Our ability to meet some of these requirements is now questionable, and, as a consequence, the vitality and the future of the Space and Earth Science enterprise are threatened.

Signals of Stress and Change

The systemic difficulties which have developed in the Space and Earth Science Program have, at least in part, resulted from the facts that:

- The Space and Earth Sciences have widened their horizons. The accomplishments of science in space have opened a broad frontier of new and fundamental scientific possibilities, have prepared the way for a variety of practical benefits, and now promise even greater rewards from the continued exploration of the Earth and the heavens. New disciplines are realizing the benefits of science in space. The successes of the Space and Earth Sciences could be but the dawn of a bright future. However, there are many more worthwhile opportunities for exploration than can be accommodated by the resources expected to be available.
- Space technology required for new advances is more sophisticated and more costly. The advances in space research have mandated the development of observing systems capable of greater temporal, spatial, and spectral resolution. Such systems are technologically complex, heavier, require more power, and produce data at rates that challenge current capabilities. But if science is to advance, the technological pace must be maintained, and the resulting increased costs have to be accommodated.
- Interactions between an increasingly constrained NASA and a larger and more diverse scientific community are creating serious stresses. Within the space research community, with its many components and interests, there is intense competition and tension. Strong proponents see lost opportunities; they fear their future may be one of delay and decay rather than stimulating accomplishment. The complexity of the current endeavors raises concerns about the retention and stimulation of the individual creativity and initiative essential to scientific progress.
- Assured access to space is no longer obvious. The number of flight opportunities for Space and Earth Science payloads has gradually decreased. While this trend is due in part to the widening scientific horizons and more diverse research community, it also is the result of not matching the launch vehicles to the purposes of the scientific missions. Space and Earth Science cannot advance without assured access to space.

The character of the Space and Earth Science Program is changing. If the program is to be guided properly, conscious steps must be taken to manage the change. Awareness of the issues and trends must be the key first step in proceeding in a more systematic fashion.

Recommendations

In order to foster the vitality that is at the heart of a productive Space and Earth Science Program, SESAC presents the following recommendations.

1. The Space and Earth Science Program must continue to incorporate a diverse range of activities, participants, and facilities. (Chapters 4, 5, and 7)* The vitality of the program conducted by NASA's Office of Space Science and Applications (OSSA) rests in the availability of a range of activities and facilities. Low-cost suborbital missions are essential for addressing certain scientific questions on a short time scale, for technology development, and for graduate education. Moderate scale missions focus on specialized scientific issues. Major facility-class missions have become essential for answering fundamental scientific questions in each of the Space and Earth Science disciplines and must be provided in turn on an appropriate schedule. The OSSA Research and Analysis program is the foundation on which the vitality of the Space and Earth Sciences depends. It must be strengthened in a number of significant ways and protected from funding fluctuations.

Cooperation and collaboration among all components of the Space and Earth Science community—NASA Headquarters, the NASA Centers, the universities, industry, other Government agencies and Federal laboratories, and international partners—are the key to effectively conceiving, planning, constructing, and managing space missions. Each component of the space science infrastructure provides unique capabilities and perspectives, and this diversity must be maintained. In this report we reaffirm the significance of the several roles played by each component of the space research community and recommend that increasing cooperation be promoted. NASA should, with the assistance of the entire research community, explore the potential advantages of new organizational structures, including consortia and formal academic and industrial partnerships. Increasing capabilities outside the United States are potential sources of new opportunities. We praise the ongoing efforts between NASA and the European Space Agency (ESA) to establish a policy of reciprocity of flight opportunities. We note the valuable opportunities offered by the Japanese Institute of Space and Astronautical Science (ISAS) for U.S. participation in the Geotail and High Energy Solar Physics missions. Other possibilities for bilateral and multilateral cooperation with other space-faring nations also exist. We urge NASA to pursue and take full advantage of collaborative and reciprocal opportunities which may arise.

^{*}The chapters referred to following each statement contain the arguments and discussions which have led to these recommendations.

- 2. The scientific requirements of a particular mission must be the dominant factor in selecting the launch vehicle, instruments, and spacecraft to be employed. (Chapters 4 and 7) It is imperative to adopt the most appropriate launch vehicle for each program. NASA must reintroduce expendable launch vehicles into the fleet. Manned space flight must be used only when a manned capability is essential for meeting scientific requirements. But having choices available for launch is only one step in optimizing the program. Proper matching of instruments with spacecraft capabilities must be done on the basis of the scientific needs of the mission, not on the basis of exploiting an available facility. There must not be confusion between ends and means. This will become an increasingly significant point as we move into the era of the Space Station. Thoughtful preparations must be made for the utilization of the Space Station. Use of the Station should begin with simple experiments, which then evolve toward more complex ones as the Station's capabilities become better understood. Science payloads should not be selected merely on the basis of the availability of space on the Station. There are established mechanisms for selecting payloads on the basis of their scientific merit, and this philosophy must be maintained for Station or platform manifesting. The Space Station will be only one of a range of tools available to OSSA. OSSA should select what science is to be done before selecting the most appropriate mode of performing the experiments, whether that be as a Station or Shuttle payload, or an instrument on a unique free-flying spacecraft, a spaceprobe, or a servicable, retrievable platform.
- 3. All aspects of the Space and Earth Science Program, and their total requirements for resources, must be thoroughly and realistically understood through rigorous planning. (Chapter 7) NASA management and the research community must make efforts to optimize the current utilization of resources and talents. OSSA should reexamine its approach toward implementation of flight projects with the intent of reducing overall mission costs. This effort should include use of similar, but appropriately modified, spacecraft for several missions; reducing requirements for documentation while reappraising the level of reliability needed for each mission; and more realistically matching mission needs with spacecraft and instrument capabilities. Once a project has been started it must be completed on the most cost effective schedule. A flight project should not be started until the launch or carrier vehicle is assured and a clear understanding exists of the risks associated with any necessary new technology connected with the carrier. OSSA should also consider broader implementation of the current funding process applied to the Explorer program in which missions are developed and launched a few at a time within a fixed funding envelope.

Especially for larger missions, runout costs, including operations and data analysis costs, must be well understood before a project is officially started, and if a major delay or descoping appears necessary, then OSSA must address the issue of whether the program is still viable and retains its original priority. Large cost overruns cannot be tolerated. Because of limited resources, careful choices should

be made about the number of projects which are in the definition and design stage (Phase B) at any given time. Just as the number of Phase B projects should be limited to those with a reasonable expectation of being started, so should Announcements of Opportunity only be released for those projects that have a reasonable prospect of entering the development phase with a few years following investigator selection.

4. Carefully specified criteria must be used in setting priorities and deciding among proposed major space research projects or missions. (Chapter 6) The Space and Earth Science Program consists of a large number of research and data analysis projects, of suborbital experiments, and of a family of space missions ranging from the small and short-lived, to the very large, permanent facilities. All elements of this program must be melded into a coherent whole. Moreover, the selections of the major facility missions that become the center piece activities of the individual science disciplines are especially significant because such choices involve substantial near-term funding requirements, determine the long-term direction of whole fields of research, and obligate funds well into the future. Careful specification of the criteria for evaluating the scientific merit, programmatic implications, and societal benefits of proposed new Space and Earth Science projects or missions is essential to make effective decisions. We propose such criteria, formulated as questions, and urge that the criteria be applied by all who are involved in making the difficult decisions that shape the future of the Space and Earth Science Program.

A Broader Issue

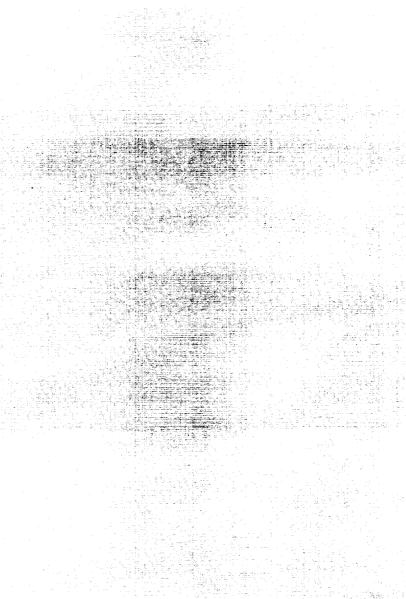
The proud advance of the Space and Earth Sciences in the first quarter-century of the modern space age have created many more exciting opportunities for science in space than can be accommodated by the present budget of the NASA program. Thus, the critical question to be faced is whether the Agency should be reponsive to scientific imperatives or curtail its efforts to fit within a budget determined on the basis of extra-scientific criteria. In either case, maintaining the focus and effectiveness of the program is essential to providing the greatest possible scientific return, thereby justifying public support. Regardless of the size of the program, resources must be effectively utilized to produce the highest quality scientific results.

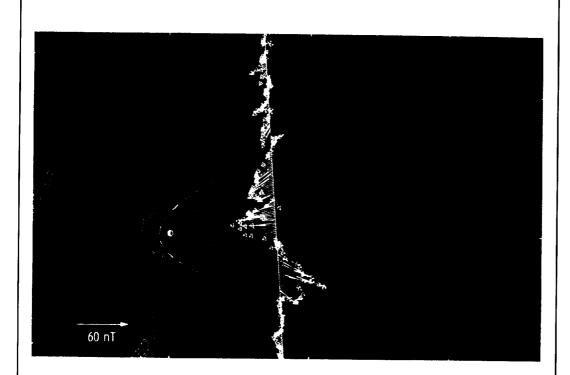
If additional resources are not available, then there are only two options: either progress in all of the Space and Earth Sciences must be delayed or else some of the disciplines must be assigned a substantially higher priority to proceed at an optimum pace. In either case, some disciplines will perceive a future that is bleak at best and will lose vitality.

The relevant elements of the Executive Branch and Congress must participate in continuing discussions on the future of the NASA Space and Earth Science Program in order to foster stability, predictability, and realistic expectations. Decisions and choices must be made. Once decisions are made, programs should proceed on a firm schedule. The continued health of the research program requires predictability in continued support from year to year. Graduate students cannot be encouraged to select a career in space research if they see fluctuations in the research base or if projects are started, postponed, restarted, delayed, refocussed, and possibly canceled. Obviously, senior scientists also cannot function in such an unstable environment. We must promote a more rational use of human resources. Above all, whatever the actual levels of funding for the various programs, a certain level of stability must be imposed across the spectrum of research activities in order to provide a predictable program with realistic expectations.

Restricting access to new knowledge through parsimony is not in the nation's long-term interest. Science, by its very nature, promotes progress. Progress in science necessarily leads to further scientific endeavors, greater achievement, as well as greater costs. The direct and indirect rewards of effectively conducted research provide the increased productivity to finance the continued growth of science. The past three decades have clearly shown that the Space and Earth Sciences, carefully managed and carefully nurtured, can be among the nation's most rewarding investments.

We must, therefore, move ahead with our voyage into space, to observe and measure our Earth and its environment from great heights, to visit and explore the distant planets, to probe the depths of our Galaxy where stars are born and stars die, to search the outermost reaches of the universe to learn about our cosmic origins, to fathom the deeper laws of nature, to investigate the origins of life, and thus, to find our place in the greater design of the world around us. This is where America has made major intellectual contributions in this century and should also continue to do so. Let us press forward.





Chapter 1:

The Crisis in Space and Earth Science

NASA's mandate, as specified by the National Aeronautics and Space Act of 1958, includes the conduct of space activities which contribute materially to the expansion of human knowledge of phenomena in the atmosphere and space and "to the preservation of the role of the United States as a leader in aeronautical and space science and technology." A vital Space and Earth Science Program is required to achieve these goals. At present, the vitality of that program and, thus, NASA's ability to meet its charter and to exercise leadership are seriously threatened.

Even before the Challenger accident and the resulting hiatus in the space program it was becoming clear that the nature of the Space and Earth Science Program was changing and that major stresses were developing as a result of those changes. Within the scientific community there was a growing sense of unease and frustration with the program's progress. Although many members of the scientific and science policy communities were aware of some of the individual changes, it was also clear that the nature and implications of what was happening had never been thoroughly and systematically examined. As the result of a number of trends, it appeared that a major transition was taking place in the nature of the Space and Earth Science Program, but it seemed that this transition was taking place more by accident than as a matter of conscious policy. Decisions were also being made which had longterm consequences concerning how the Space and Earth Science Program would be conducted, but the consequences of those decisions were largely

quences of those decisions were largely unexamined. More and more missions were being identified as candidates for "New Starts" at a time when prospects for the start of such missions were uncertain. The competition among prospective missions was escalating to a counter productive level. There was a sense that the future health of entire disciplines depended on single decisions. The emergence of the Space Station as a major NASA initiative was raising questions as to whether NASA's science program would be reoriented around this facility. Questions concerning priorities were being raised without any obvious way to address those questions systematically. More and more scientific groups seemed to be competing for fewer and fewer flight opportunities. The push to start major new missions seemed to be leading, at the same time, to an erosion in those smaller-scale, less glamorous, less visible activities, which, in many ways, formed the foundation of the program and ensured that the scientific return from the major missions really justified the investment. All of these difficulties have now been seriously exacerbated as the result of the Challenger accident.

Two years ago, as a result of many of these perceptions, the Space and Earth Science Advisory Committee (SESAC) embarked on a wide-ranging examination of programmatic issues facing the U.S. Space and Earth Science Program. The intent of this study was to examine the changes that were taking place, to understand the implications of those changes, and to make recommendations for a long-term, productive program in the Space and

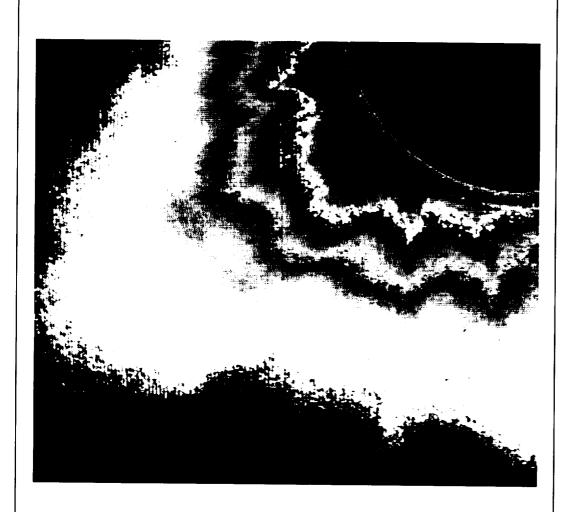
Earth Sciences. A major goal of the work was to try to develop a more rational process for making decisions, especially decisions concerning major new initiatives. The fundamental issue underlying this effort by SESAC was to examine how the use of the limited available resources could be optimized to put together the best possible science program. This report is the result of those deliberations.

The trends, developments, and forces that have been acting on the Space and Earth Science Program, producing both stresses and changes, are catalogued and discussed in Chapter 2. Having identified the stresses and changes, the report next turns to the question of where the Space and Earth Science Program should go from here. The requirements for maintaining the vitality of the Space and Earth Science Program are described in Chapter 3. Chapters 4 and 5 deal in more detail with the two most fundamental aspects of ensuring program vitality—the spectrum of activities required to carry out a productive program and the role of individuals and institutions in that program. Chapter 6 deals with the decision-making process and presents a systematic methodology for making rational choices among major initiatives in the Space and Earth Sciences. Special attention has been given to the decision-making process because the decision to proceed with a given major

New Start not only involves significant commitments of near-term resources but, in many cases, substantial long-term commitments as well. Chapter 7 deals with specific recommendations for optimizing the scientific return from available resources. Concluding reflections on the history of the Space and Earth Science Program and broad recommendations for the future are given in Chapter 8.

The issues dealt with throughout this report are complex and it has not been possible to arrive at definitive conclusions or recommendations concerning all of them. It is SESAC's hope, however, that an examination of these issues will provide NASA management, the Office of Management and Budget, the Office of Science and Technology Policy, the Congress, the scientific community, and the American public with an awareness of the fundamental changes that have occurred and the serious difficulties now facing the Space and Earth Science Program.

Much of the work of this study had been completed before the Challenger accident. Many of the issues that had been identified by that time have not fundamentally changed as a result of the accident, but, as will become evident throughout this report, they have been dramatically compounded. The need to manage stress and change has been transformed into the need to manage crisis.



Chapter 2:

Signals of Change and Stress

Over at least the past decade it has become increasingly apparent that the nature of the Space and Earth Science Program was changing in ways which were producing increasing stress on both the participants and the program. In addition to intrinsic changes, other events, both within and external to NASA, were also contributing substantially to the stresses. As will be discussed below, some of the problems were inevitable consequences of successes in the program. However, the Challenger accident dealt a devastating blow to all U.S. space activities and created a crisis for the Space and Earth Sciences. This Chapter examines the major developments and forces which have been acting on the program. The goal of this examination is to analyze the current nature and dynamics of the program in order to provide a basis for considering the steps required to ensure that the Nation can have a longterm future in the Space and Earth Sciences.

a. The Character of the Space and Earth Sciences is Changing

The research environment and requirements for the Space and Earth Sciences are changing as a result of developments in the various scientific disciplines themselves, together with advances in technology. Major important changes include a transition to "big science" and the accompanying requirements for long-term resource commitments, the possible changing roles of participating institutions, and

the emergence of new computational capabilities and needs.

The trend toward big science. The days of simple science in space are largely over. This is often lamented by those who yearn for the early days of the space program and the 10 kilogram single Principal Investigator experiment flown on a small Earth-orbiting spacecraft. As a result of scientific developments, the trend of space research is now towards "big science" and the use of major facilities. The major astronomy and astrophysics initiatives recommended for the 1980's and 1990's are the "Great Observatories," major facility-class flight missions whose total runout costs would be comparable to, or could even exceed, the yearly OSSA budget. Important planned major initiatives in solar and space physics will require multispacecraft observations in near-Earth space to supply the needed scientific data. Planetary missions are now often facility-class missions with sophisticated instruments designed and operated by facility teams. Major advances in understanding in the Earth sciences will require facility-class missions for sophisticated, long-term global observations of planet Earth.

This is not to say that small missions and suborbital science opportunities are unnecessary and will be gone forever from NASA science. Indeed not. But the roles of these activities and their contributions to the total scientific program must be carefully assessed and defined for each scientific discipline. The necessary mix of activities is strongly discipline-dependent.

It is important to recognize that

the changes in the Space and Earth Sciences are not dissimilar to those occurring in other, vastly different, areas of science. For example, the future frontiers of particle physics lie in the energy range only achievable by the proposed mammoth superconducting supercollider facility even though smaller experiments, such as those searching for the magnetic monopole, are also clearly important. The future of fusion physics appears to lie in the Tokomak Fusion Test Reactor (TFTR). At the same time, smaller laboratoryscale reconnection experiments are also in progress, making research advances important for both the TFTR and for space plasma physics. Even the biology community—one of the last major bastions of small science investigations—is vigorously debating the wisdom of a major effort (of the financial scale of a Great Observatory) for sequencing the human genome.

Large facilities imply long-term resource commitments. A decision to proceed with a specific major facility is also a commitment to a specific scientific direction over a long future in a given discipline. This also implies a long-term funding commitment. A decision to proceed with a major facility then, in some sense, mortgages the scientific future not only for the benefitting discipline but also for the other disciplines which, if the overall funding envelope is fixed or only slowly increasing, thereby find themselves with fewer resources for science planning and mission development. Thus, support for the operation of several long-term facilities simultaneously could represent a significant future lien on the resources of the Space and Earth Science Program.

Potential changes in individual and institutional roles. Because of the trend towards "big science," the skills required for the execution of many NASA projects differ from those in the past. Increasingly, individual space researchers contribute their skills as a relatively small component of a larger team effort. Again, space research is not unique, the situation is even more pronounced in elementary particle physics. The preservation of both the breadth and depth of individual skills, particularly in the development of innovative instrumentation, is difficult to achieve. At the same time, expanding and increasingly accessible data bases and computer facilities create both need and opportunities for new individual skills in data analysis, computation, simulation, and theory. The trend towards "big science" and facilityclass missions has also produced stresses in the participating institutions. Much of the construction of space hardware is often beyond the capability of a single institution, particularly smaller university departments. Furthermore, it can be asked whether involvement in some types of hardware work, having major administrative and management commitments, is consistent with the educational goals of a university. As a result, investigators have found that multiinstitutional collaborations and consortia are often necessary to conceive, design, and construct major components of space hardware. Multiinstitutional arrangements are also often found to be desirable for data handling and interpretation because no single institution always has the breadth of expertise required to interpret data from instruments studying a complex scientific problem. Large, specialized pieces of laboratory equipment cannot

always be duplicated in each institution, a situation which requires interinstitutional sharing of such resources. The major issue—and not just relative to space research—is how to accommodate the reality of the shifts to big science while preserving the capabilities for individual creativity and the stimulation arising from individual and group accomplishments.

The emergence of expanding computational capabilities and needs. The planned downward-looking and outward-observing space facilities will create huge data bases over their lifetimes. Several of the high resolution instruments of the planned polar orbiting Earth Observing System (EOS) mission will generate on the order of 10^{13} bits per day. The data from the Hubble Space Telescope (HST) will be, on the average, the equivalent of 20 pictures per day, each 1600x1600x16 bits deep (10⁹ data bits per day). Other disciplines, such as space plasma physics, which involve in situ measurements of the actual space environment, will also greatly increase the rate and complexity of their acquired data. The steady and rapid growth in computational capability also makes it possible to carry out increasingly ambitious simulations of Space and Earth Science processes. The changes in computational needs and capabilities will significantly affect individual and institutional Space and Earth Science research activities. Considerations ranging from data handling, to algorithm development, to sophisticated simulations must be addressed in ways different from those of the past. These new computational capabilities and needs also may require different approaches to such areas as data distribution and archiving, remote

access to facilities and data bases, and the education of students.

b. Emergence of new spacerelated opportunities

The successes of space research have led to a widening horizon of research opportunities. The Space and Earth Science disciplines traditionally identified with the U.S. space research program-astronomy and astrophysics, solar and space physics, planetary exploration, and earth observations have identified expanding and challenging research opportunities which require a broader range of resources, including substantial new flight opportunities. The scientific questions have become more complex. The questions have evolved from simple yes/no types to detailed ones asking about processes and seeking predictions. As a result, the trend, as noted above, is toward facility-class missions in many cases.

At the same time, there are a growing number of claimants from emerging disciplines for space research resources. There are "new teams in the league," so to speak, whose science has reached the stage where space-based possibilities could now allow major advances to be made. For example, important research possibilities are emerging in space biosciences and in areas of basic physics and chemistry which can take advantage of the microgravity environment.

c. Scientific possibilities exceed available funds

Even for the traditional "teams in the league" the resources required for the research aspirations of the Space and Earth Science Program elements

exceed those likely to be available. Some estimate of the needed level of resources can be developed by first looking at the funding history of NASA overall and of the Office of Space Science and Applications (OSSA) as shown in Figure 1. The upper panel also shows the history of the OSSA percentage of the NASA budget, a percentage which has been approximately constant for more than a decade. The peaks in OSSA funding in the lower panel reflect the peaks in major flight projects, including major program thrusts in lunar science (in the mid-1960's) and in Mars science (the Viking peak in 1972-1973). The peak in 1979-1980 is due to the sum total of activities associated with completion of the High Energy Astrophysics Observatory (HEAO) program, the Landsat development, and the start of the Hubble Space Telescope and Galileo. While OSSA funding has slightly increased again in the early 1980's, large changes are not likely in the near term.

Over the past several years the various Space and Earth Science disciplines have developed carefully considered, long-term science strategies for their respective research areas. These strategies have been developed under the auspices of both the National Academy of Sciences and the NASA Advisory Council and include both recommendations concerning specific missions to accomplish the identified science objectives and projected funding requirements.

The scientific aspirations of the Space and Earth Science research communities resulting from the science strategies are illustrated in Figure 2. Three of the four figures shown are reproduced directly from the committee reports referenced in the figure

captions. In the case of the astrophysics budget projections, the funding curves were developed by NASA in response to the recommendation of the report of the Astronomy Survey Committee of the National Research Council, a report which contained ten year average budget recommendations. To see the implications of total discipline requirements, the budgetary envelopes of each of these discipline aspirations are shown in Figure 3, with each converted to constant 1985 dollars and adjusted to remove overlap in projects that were included in the reports of several committees. For comparison purposes, the level of the FY 1986 OSSA resource envelope is shown in constant dollars, which appears appropriate in the current deficit reduction climate. It should be noted that the OSSA total includes funds for the life sciences, microgravity science and applications, and communications programs, as well as the Space and Earth Sciences. The mismatch between current funding levels and the aspirations of the Space and Earth Science community is clear.

d. Fluctuations in science discipline funding

The large year to year fluctuations in the funding of flight projects, shown in Figure 1, arise because of specific, large-cost individual missions. Figure 4 illustrates that the impact of these flight project fluctuations on the funding available for individual disciplines can be quite large. Thus, when flight project funds in a discipline decline, the support of research and research activities must be borne by the research base. While the overall research base has been increasing slowly, it has decreased significantly in the traditional space sciences—astrophysics, planetary

The center panel shows the total NASA funding in terms of FY 1982 dollars, from 1960 to the present, and the comparable OSSA funding bistory. Given the organizational changes that have occurred during the years, the curves labeled here as OSSA refer to the sum of the traditional science and applications divisions of research in NASA. The bottom panel similarly presents OSSA total funding but separately plots the component flight projects and research base. Within the research base, funding of traditional space sciences (astronomy, solar-terrestrial physics, and planetary exploration) are distinguished from the funding of traditional applications areas (earth observations, life sciences, materials, and communications research). The upper panel is a plot of the OSSA percentage of the NASA budget.

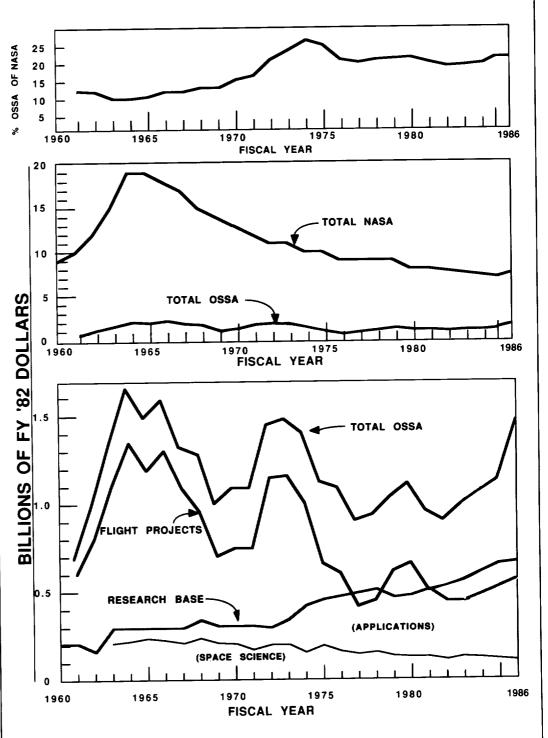
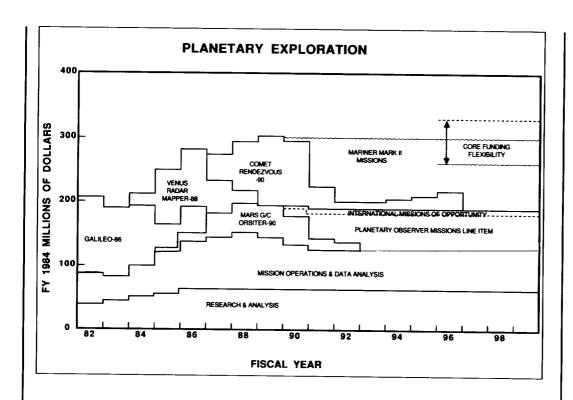


Figure 1. Funding History of NASA and the Office of Space Science and Applications (OSSA).



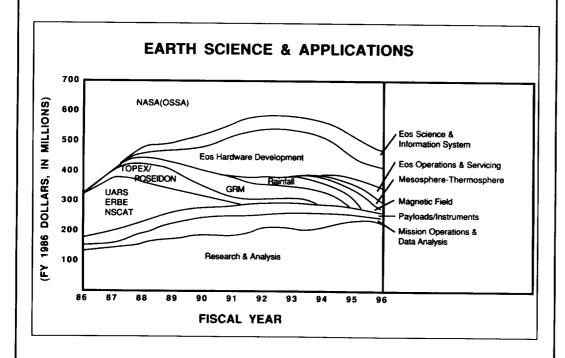
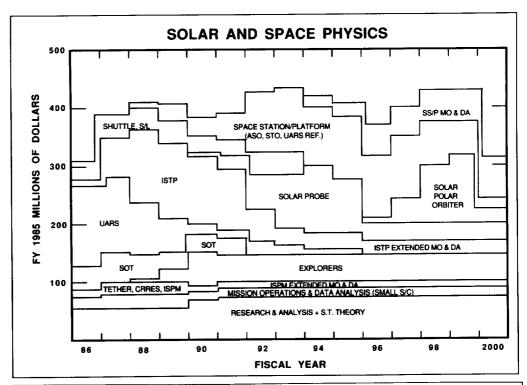


Figure 2. Aspirations of the Space and Earth Science Research Communities.

The four figures shown are reproduced from previously published documents. The Planetary Exploration panel shows the programmatic and funding aspirations as developed in 1983 by the Solar System Exploration Committee (SSEC) of the NASA Advisory Council and published in "Planetary Exploration Through the Year 2000: A Core Program." The Earth Science and Applications panel is reproduced from the 1986 publication "Earth System Science: A Program for Global Change," developed by the Earth System Science Committee (ESSC) of the NASA Advisory Council. The ESSC made recommendations and budgetary estimates to NASA, NOAA, and NSF. Reproduced bere is the NASA (OSSA) portion of the recommended program strategy. The Solar and Space Physics panel is reproduced from the 1985 National Research Council publication of the Space Science Board's Committee on Solar and

Space Physics (CSSP) report, "An Implementation Plan for Priorities in Solar System Space Physics: Executive Summary." The fourth panel, NASA Astrophysics, is reproduced from a chart developed by the NASA Astrophysics Division in response to the recommendations of the 1982 report of the Astronomy Survey Committee (ASC) of the National Academy of Sciences, "Astronomy and Astrophysics for the 1980's: Volume I." The ASC report contained only ten year budget recommendations, so NASA developed the figure bere in order to show what was required to implement the ASC recommendations. Note that (1) the four panels are not in consistent year dollars, but bave been reproduced as in the original, (2) there is duplication in aspirations between the ESSC and CSSP reports in the UARS project; and (3) duplication exists between the CSSP and ASC reports in solar physics, most notably the Solar Optical Telescope project.



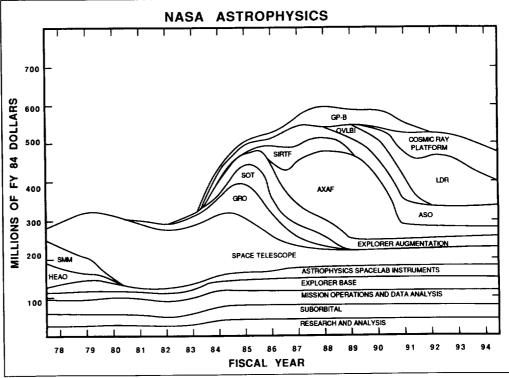


Figure 2. Aspirations of the Space and Earth Science Research Communities (continued).

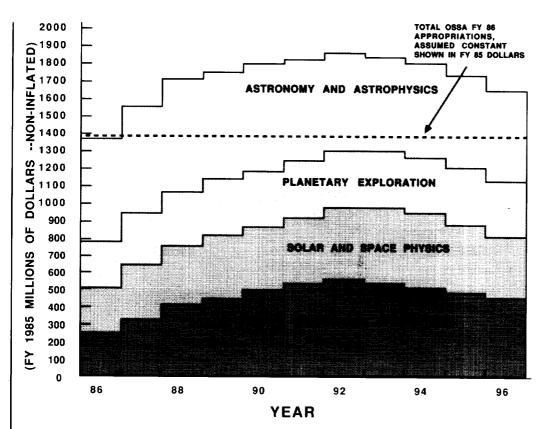


Figure 3. The Summation of Discipline Aspirations.

exploration, and solar and space physics. As a result, the research base has not been able to absorb all of the demands made upon it. Consequently, a widely fluctuating flight project budget creates ever increasing stresses on the research base and removes the stability that research and analysis funds can provide.

e. Expansion of the Space and Earth Science community

The Space and Earth Science research community has greatly expanded in the last two decades, an expansion encouraged by the perceived exciting scientific opportunities discussed earlier. While precise demographic data are not available for all disciplines which comprise the Space and Earth

Sciences, the expansion of the space research community appears to roughly parallel the expansion in the period from 1960 to the mid-1970's of Ph.D. degrees awarded. The data on physics Ph.D.'s is readily available. From less than 600 physics Ph.D. degrees awarded in 1960, the total rose to over 1,500 degrees in 1971. The yearly awards have now dropped to about 900 or so. From 1964 to 1983, the number of Ph.D.'s in astrophysics, one of the disciplines of Space and Earth Sciences. has ranged from a low of 50 to a high of 77, with a median over this time period of about 65 per year ("Physics Through the 1990's," 1986). The production rate has far exceeded the retirements from the field, so the total pool of active scientists increases each year

In this figure, the envelopes of the four charts in Figure 2 showing science discipline aspirations bave been converted to uniform 1985 dollars and plotted together in summation. The overlap in programs pointed out in Figure 2 has been partitioned so that there is no duplication in the summation of the funding estimates. Also shown on this figure is the total OSSA FY 1986 Congressional appropriation (converted to 1985 dollars for comparison purposes). It is shown as a reference level only and is plotted as if constant in future years. The 1986 level was chosen because this final appropriation level represents the results of impacts due to deficit reduction initiatives and the initial effects of the Challenger accident on OSSA 1986 spending. At the time of this printing. it is not possible to speculate on future changes to this funding level. It must be remembered that the total OSSA budget supports not only the science divisions of Astronomy, Planetary Exploration, and Earth Science and Applications (including solar-terrestrial physics) but also those of the Life Sciences, Microgravity Science and Applications, and Communications programs.

This figure is reproduced and updated from the 1984 Office of Science and Technology Policy (OSTP) report on "Funding Trends in NASA's Space Science Program.' It shows the raction of the total OSSA actual spending between 1975 and 1986 devoted to flight projects in the disciplines indicated. Included in astronomy are all astronomy Explorer missions, the High Energy Astrophysics Observatories (HEAO), the Gamma-Ray Observatory (GRO), and the Hubble Space Telescope (HST). All planetary missions under development during this interval are included in the planetary exploration curve. The solar-terrestrial and earth sciences disciplines include Landsat, the appropriate Explorer missions, Ulysses, the Upper Atmospheric Research Satellite, and MAGSAT. Short duration Sbuttle missions and suborbital flights (e.g., sounding rockets) are not included in the data period.

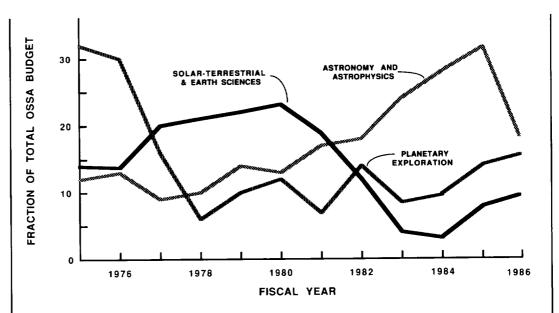


Figure 4. Fluctuation in Discipline Flight Project Funding.

by almost the number of new degree awardees.

It is more than reasonable to expect that many of these new Ph.D. recipients desire to continue their research careers. Unfortunately, with the declining research opportunities discussed below this will be increasingly difficult in the Space and Earth Sciences. Furthermore, present students and research scientists are the proteges and, in some cases, the grandproteges, of scientists who themselves remain in active competition for research funding and flight opportunities.

f. Effects of program delays and stretchouts

A significant fraction of the Space and Earth Science budget is now being consumed by delays and stretchouts of flight projects. This situation, which has been present for some time, has been greatly exacerbated by the Challenger accident. For example, the Space-

lab 2 mission budget increased from an initial \$27 million to a final cost at launch, five years later than originally planned due to Shuttle manifest slips and delays in the availability of key pieces of Spacelab hardware, of \$70 million. Another recent example is the Galileo orbiter and probe mission to Jupiter. As the launch date slipped from the original 1982 target to the 1986 opportunity, the costs rose from \$379 million to \$843 million. The additional slip resulting from the Challenger accident and from the subsequent cancellation of the Shuttle/Centaur upper stage will increase the costs yet further.

Much of the time of creative scientists is wasted as launch dates change, including the time which must then be devoted to analyzing and reanalyzing revised mission scenarios, in budgeting and rebudgeting exercises, and in planning and replanning research programs for students, colleagues, and themselves. The time from the release of

the Galileo mission Announcement of Opportunity to probe entry into the Jovian atmosphere will now most likely be nearly twenty years, more than onehalf a research career (see also the discussion below).

When launch schedules become stretched, instruments that are flown may no longer be on the forefront of experimental science and may obsolete. Resources are rarely available to update instrument technologies when a launch delay occurs. In addition, present day spacecraft systems are often so complex and interwoven that one subsystem cannot readily be changed without compromising other parts of the entire system.

g. Fewer opportunities for space flight experiments

As discussed above, there are increasing scientific requirements for research in space and more groups vying for the available resources. At the same time, there are fewer opportunities for access to space. The launch rate history compiled in Figure 5 illustrates this clearly. Launches in the 1980's consisted of a few Explorer missions which occurred in 1981, 1982, and 1984 plus a limited number of launches of the Shuttle carrying one or more scientific packages. The Shuttle has not fulfilled its early promises for ready, inexpensive access to space, although the manifest schedule, prior to the Challenger accident, was building up to an increasingly ambitious science component in the future. Research in space requires ready access to space, an access which has been continually decreasing for U.S. scientists in recent years. The combination of increasing scientific opportunities, more groups

capable of doing first class research, and a decreasing flight rate has been an increasingly severe stress on the Space and Earth Science Program.

h. The time scales of various aspects of the program are inconsistent

There are a variety of important time scales in the political, scientific, and educational spheres that bear significantly on the efficiency, or inefficiency, with which space research is pursued. In the United States, political time scales generally are four or eight years for Administrations, and two and six years for Members of the House and Senate, respectively. The annual budget cycle of the Federal Government is on a time scale that often has serious ramifications for space investigators. The yearly budget cycle does not allow a firm commitment to a specific program even after it is approved during one cycle. In some recent years, the budget has changed even more frequently than yearly. The lack of a commitment can result in program cancellations and/or delays. Planning becomes very difficult. It is also hard to make firm agreements toward foreign collaborations.

In the scientific realm, members of science advisory committees generally have three-year terms. Discipline-wide studies of scientific priorities typically occur every decade. Time scales for scientific considerations can be determined by natural phenomena such as the need for observations at a particular phase of the cycle of solar activity. Similarly, the requirement for gravity-assisted maneuvers depends on particularly favorable alignment of

This histogram shows successful Space and Earth Science launches by year from 1959 to the present. Each unit box represents one payload, whether launched by an expendable vehicle or by the Shuttle. Criteria for inclusion in this figure did not include the size or cost of the mission. That is, all expendable or Shuttle launches which successfully carried or deployed at least one Space and Earth Science mission component have been included. Multiple payloads carried on a single launch are counted as one launch. Thus, the figure represents flight opportunities per year.

LAUNCH RATE

SUCCESSFUL SPACE & EARTH SCIENCE LAUNCHES BY YEAR

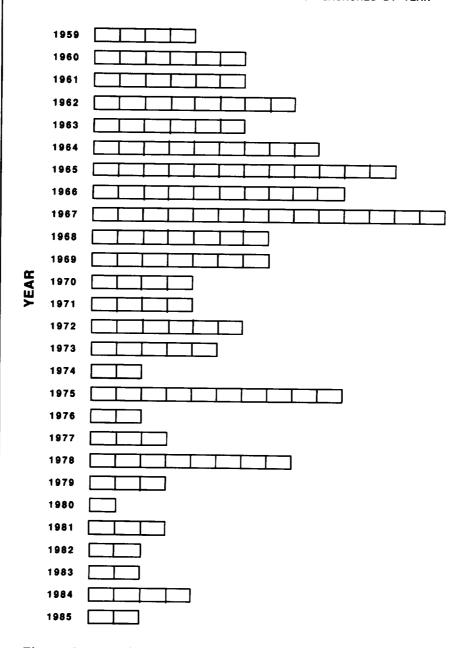


Figure 5. Launch Rate.

planets. Scientific time scales can also depend on advances achieved in the various disciplines. As research problems in one area are solved, new problems emerge which attract the attention and interest of researchers and advisors; major redirections in a discipline seem to occur about once a decade in a vigorous discipline. New technology evolves on other time scales, but frequently lags behind the concept development which needs the technology. The development time scale for new projects from scientific concept, to study groups, to Announcement of Opportunity, to prioritization, to selection, to construction, to launch is highly variable, not often orderly, and can range from a few years to a decade or even more (Figure 6). As is evident from this Figure, the development time for missions has become stretched out further and further. The longer time scales are beginning to be a significant fraction of a scientific career.

In the academic realm, typical graduate careers span four to six years, with two to three years devoted to the dissertation. A young space scientist, fresh from the Ph.D. program, has perhaps five years to establish a reputation sufficient to earn a permanent research or academic position, which will then yield three or more decades of high productivity. The prospects available to such a scientist are significantly dependent upon events occurring within both the political and the scientific time scales.

There are obviously serious inconsistencies between the natural time scales of the various components that can determine the success or failure of a space research program. A stable national program cannot be easily or

rationally developed in such an environment.

i. Emergence of strong capabilities in other nations

From the very beginning of NASA, international activities have played a significant role in the Agency's programs. Indeed, the foundation for international endeavors are found in the legislation which created NASAthe Space Act of 1958—which directed the Agency to conduct its activities "...so as to contribute materially to...cooperation by the United States and other nations and groups of nations." More than 1,000 agreements involving some 135 countries and international organizations have been made by NASA to the present time (Logsdon, 1984; Rosendhal. 1986).

However, while international cooperation will continue to be an important part of NASA's total program, the nature of the foreign partnerships and the capabilities of foreign partners are becoming very different than in the early days of the space program. The present equality of technical ability was strikingly evident in the success of the international flotilla of spacecraft which pursued and studied Halley's Comet. This balance of capability means that future cooperation will be undertaken on a much more equal basis than has usually been the case in the past. As a result, foreign partners will insist on deeper involvement in the planning, management, and operation of missions.

Increases in space research budgets abroad, such as those approved last year by the European Space Agency, also imply that potential foreign partners may, in the future, be able to underFollowing the loss of the Challenger, mission launches are identified by an arrow at the current most probable launch date as specified on the recently published NASA manifest. Nonapproved programs for which an AO has been released are denoted by a dashed line ending at the launch schedule expected if New Start selection occurs according to current planning. Redefinition of missions is also indicated for those cases where a major mission rescoping bas taken place.

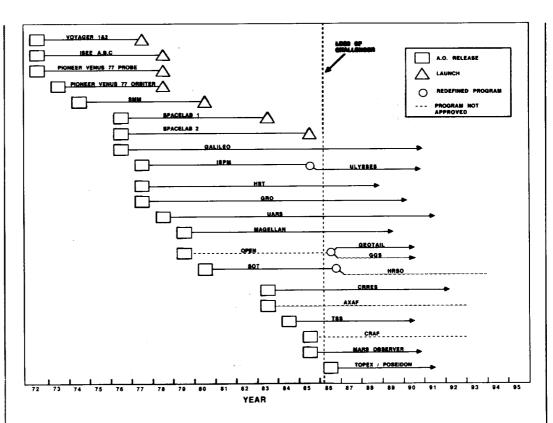


Figure 6. History of the Time Interval Between Release of an Announcement of Opportunity (AO) and Launch of Major OSSA Missions.

take more expansive and/or ambitious programs. This could result in greater opportunities for cooperation.

New foreign collaborations may continue to develop in which the U.S. is a minor partner or not a partner at all. Such instances have existed in the past on a more limited scale (particularly the French/USSR collaborations), but they may become more prevalent in the future. In contrast to the situation of a decade or two ago, the U.S. is no longer in the position of having a substantial influence on every major development in space research.

j. Space research and the advent of the Space Station

The advent of the Space Station as a major U.S. national initiative is a reality that the research community will have to recognize. The Space Station represents a major commitment by NASA that will place additional stresses on resources in a budget that already has little, if any, margin. In addition, the existence of the Space Station could well place pressures on NASA for tailoring more of its programs toward utilization of the Station than scientific imperative might ordinarily warrant.

k. Federal budget process and prospects

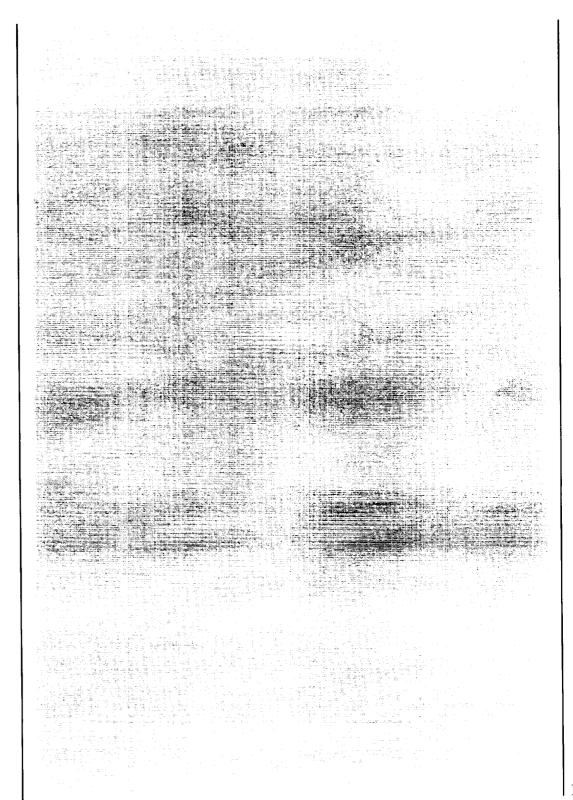
The Space and Earth Science Program is not isolated from the general economic climate-national and international—in which NASA is operating. In the U.S., it is clear from actions taken over the last year or more by the President and the Congress that a major effort is underway to try to balance the budget in the United States during the next five years. This move to balance the budget could result in an essentially level budget for NASA as a whole during this period, a prospect which, in turn, would significantly limit prospects for more activities in all areas of the NASA program. The Challenger accident has greatly complicated all fiscal considerations. There are financial impacts associated with program delays plus resources required for a new orbiter. Finally, the budget balancing process introduces large instabilities into Congressional and OMB considerations and funding decisions and often seems to make rational research planning nearly impossible.

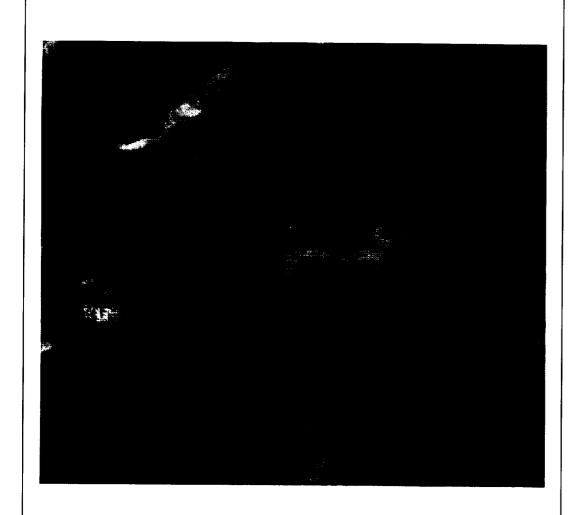
1. Effect of the Challenger accident

The Challenger accident was a serious blow to U.S. aspirations in space

research. Loss of the Challenger and the subsequent grounding of the fleet is placing severe pressures on the Agency budget as NASA proceeds to build a fourth orbiter; to maintain, for the hiatus, facilities and completed missions ready for flight; and to prepare for the future. A two year or longer delay in access to space is certain. The means, methods, and possibilities of acquiring alternative launch vehicles to the Shuttle for some science missions are very uncertain at present. The demands on Shuttle availability after resumption of flight also appear to be so severe that science flight possibilities will likely be very constrained. Infrequent access to Shuttle and Spacelab after flight resumption will affect the preparations for experiments to be flown on the Space Station.

As the above list makes clear, the U.S. Space and Earth Science Program was coming under increasing strain and uncertainty by the end of 1985. The occurrence of the tragic Challenger accident in early 1986 was devastating for all of the United States aspirations in space. For the Space and Earth Science Program the accident transformed a program under severe stress to a program in crisis.





Chapter 3:

Components of a Vital Science

The many problems and stresses summarized in the previous Chapter raise serious questions about the vitality of NASA's Space and Earth Science Program and its future direction. Before any recommendations can be made as to what must now be done, it is necessary to ask what is required to ensure the vitality of NASA's scientific program. In this Chapter we examine the necessary ingredients of a vital science.

If a branch of science is to be vital, it must have five components. First, a vital science must address stimulating questions about the fundamental processes which affect humanity, earth, or the universe. A continued availability of results from observations and experiments is required to provide a basis for formulating answers to the stimulating questions. Theories and models are necessary to suggest the relevant observations to be made, to analyze the results from the observations and experiments, and then to be modified themselves by the new empirical results. A corps of talented and dedicated scientists is essential to ask the right questions, to develop appropriate theories and models, and to design and perform effective experiments. Finally, a science discipline must have a perceived future if it is to attract and maintain the necessary cadre of qualified personnel.

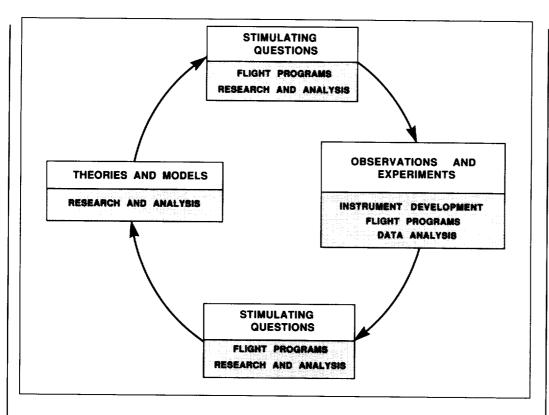
These five ingredients of a vital science are all essential and clearly are also closely interrelated. The continuous cycle which leads from stimulating questions, to observations and experiments, to more questions, to theory and models, and back to new stimulat-

ing questions is illustrated in Figure 7. The engine that drives this cycle is the corps of talented and dedicated people. The fuel for the engine, the element that inspires these people, is a perceived future.

These elements of a vital science are common to all branches of science. We now examine how they specifically apply to the Space and Earth Science Program.

Stimulating questions. The Space and Earth Sciences strive to answer questions such as: Will the universe expand forever, or is it gravitationally closed? How do stars and planets form? Did life ever exist on Mars? What is the future course of Earth's climate? And so on. Although some disciplines of space science are more mature than others, stimulating questions abound in all the disciplines, and new enthusiasm and concepts for future exploration constantly arise. Space research has become one of the major scientific activities in the United States. The reason for this rise can be traced, in part, to the abundance of stimulating questions about our environment and place in the universe that have surfaced owing to numerous successful space research missions. Observations and measurements from space have revolutionized many areas of science and have given rise to new scientific endeavors undreamed of a generation ago.

Observations and experiments. There must be a steady flow of experiments and observations. A continuing supply of new data is absolutely necessary to ensure that questions can receive



Stimulating questions lead to observations and experimentation which surface more questions, which leads to new theories and models which produce more stimulating questions. The components of the Space and Earth Science Program which support the various steps of the cycle are identified in each box.

Figure 7. The Continuous Cycle of a Vital Science.

accurate answers; new observations also lead to fresh and stimulating new questions. There must be frequent access to space. The ability to obtain full value from the observational data, which can be very expensive to obtain in the Space and Earth Sciences, also requires the development of new techniques for space instrumentation as well as for data analysis and handling.

Observations and experiments in the Space and Earth Sciences can be usefully conceived of as taking place in two phases: an exploration and discovery phase and a reconnaissance and observation phase. During the exploration and discovery phase, scientists search, perhaps guided by intuition, for new phenomena or for novel ways of viewing known phenomena. True discovery can be anticipated but never

planned: many of the most notable discoveries of science in general and space research in particular have been surprises. In seeking new discoveries, one can only aim well, be prepared, and stay alert. Once a discovery is made, reconnaissance, systematic observation, and analysis begin with the goal of seeking understanding and looking for relationships to other objects or processes. This stage of development of science is more systematic and the requirements for further advance can be predicted with some accuracy. But nurturing observations alone, unguided or unconstrained by theory, is inadequate.

Theory and models. Through development of theories and qualitative or quantitative models, scientists

attempt to consolidate and reconcile the empirical evidence gathered from observation. Good theory will arise almost naturally out of a base of good observations. Observation and experiment validate theoretical predictions or lead to creation of new theories. The new theories, in turn, drive the need for more data or experimentation in order to test the theories. Thus, the programmatic focus must be on a unified scientific effort—not just on space hardware development alone, but rather on a comprehensive set of activities which include research and analysis, development of laboratory facilities and experimentation, and the availability of up-to-date computing facilities, as well as the construction of appropriate space flight hardware. Nurturing theoretical capabilities alone while neglecting to maintain a steady flow of new observations is also inadequate. Modeling is not a final goal of a research program. Models are tools for integrating pieces of theory to study a complex system, each of whose components may be individually well understood, in order to gain a clear understanding of how the components function and interact within the whole system.

Talented and dedicated people. Without the participation and availability of the right people, there is no science. The most important compo-

nent in science is the talented and dedicated people who are curious about nature and are compelled to satisfy their curiosity. People become scientists when they acquire a command of existing knowledge and have the skills, plus opportunities, to make original contributions to the body of knowledge. Thus, a strong educational system is an essential component of a vital science without which scientific progress would come to a rapid halt. It is most important to ensure that students are being broadly educated in concepts and skills which will be useful throughout their careers, since the work they do later may be quite different from the research done in graduate school.

A perceived future. In any vital science, a perceived future is essential. The research field must have questions which are perceived to be important and interesting, not only by those active in the field, but by those outside as well, including the public at large. It is not sufficient that new observations elaborate on existing knowledge; they should promise new questions, new avenues of research, and the possibility of redirection of the science as well. However, to have a perceived future also requires stable, predictable support and funding. Stability of support may be even more important than the level of support.

In 1807, Thomas Jefferson was mindful of the need for scientific research to reduce the hazards of maritime navigation. After seeking advice from the scientific community, he asked the Swiss geodesist Ferdinand Hassler to head America's first governmental scientific agency, the Coast Survey. Before Hassler accepted

this role, he made several demands. These included an assurance of *long-term support* and assurance of the *flexibility to exploit new scientific opportunities* as they arose. Hassler's demands were met, and the Coast Survey, under his leadership, proved to be a highly successful undertaking. Hassler's principles are still valid underpinnings for Government supported science.

Keeping talented researchers dedicated to NASA's program requires some assurance that their projects can be carried through to completion. The directions and the support of their research cannot be highly erratic.

The above five ingredients of a vital science are closely interrelated. The next two chapters of this report deal with two specific requirements: the need for a diverse range of research opportunities and the need for talented people and appropriate institutions. The issue of a perceived future has

many aspects. In part it is embodied in the decision-making mechanism for the major program initiatives (Chapter 6). An important step towards program stability also involves an optimization of resources (Chapter 7) so that maximum use can be made of existing opportunities, and people can make the best use of their talents. Responsible recognition and understanding of the requirements for the vitality of research in space can lead to a solid foundation for a truly promising future for Space and Earth Science research—a future of which we can all be proud.

NINTH CONGRESS OF THE UNITED STATES.

At the Second Session.

Begun and held at the city of Washington, in the territory of Columbia, on Monday the first of December, one thousand eight hundred and six.

AN ACT to provide for surveying the coasts of the United States.

The it exacted by the Senate and House of Representatives of the United States of America, in Congress assembled, that the projectual of this Hould Matter that the projectual of this Hould Matter, in which whall the submit of the States of the United Matter, in which whall the submit of the whords, with the rends of places of anchorage, within liverily leagues of any part of the whore of the United Matter.

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ICO And be it firsthen exacted, that it shall be lawful for the president of the United States, to course such examinations and observations to be made, with report light Languis bank, and any other bank or shoot, and the sametings and currents beyond the distances;

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Sec. to And be it feether excelent, that for manying this act cuts effect their shall be, and hinchy is appropriated a sum not exceed fully thousand dellars to be paid out of any manies in thicken sury, not otherwise appropriated.

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John Ben Ch

Copy of the original Congressional Act authorizing a survey of the coasts of the United States.



Chapter 4:

Requirements of a Vital Space and Earth Science Program — The Need for a Range of Research Opportunities

As described in Chapter 3, two of the central requirements which must be satisfied if a branch of science is to remain vigorous are:

- A continuing flow of observations and experiments
- Theory and models to consolidate and reconcile the empirical evidence gathered from observations

Without observations and experiments there is no science. Without theory and modeling the observations and experiments cannot be understood. Both are required, neither can be absent.

The purpose of NASA's flight missions is to gather information about the universe around us. But the flights alone are not enough; if they are to be successful they must be built upon a strong research base which forms the foundation of the nation's space activities. For these reasons, the flight and the research programs need to be given commensurate priority. A fundamental necessity, of course, is an assured access to space. Without the capability to launch, there are no missions and no program.

It is especially important to understand these principles at this pivotal moment in NASA's history. The current absence of a launch capability resulting from the Challenger accident has created a crisis for the Space and Earth Sciences. NASA's previously planned flight program was developed with the expecta-

tion that Shuttle would be frequently and continuously available, and would transport all orbital missions—the great observatories as well as the more modest rocket-class experiments. Now that a significant hiatus in spacecraft launches has occurred, the planned program has been entirely disrupted. Even once Shuttle flights are resumed, many missions may be delayed for extended periods or may never fly at all due to a reduced launch frequency and/or launch capability. The Space and Earth Science Program must continue to progress during this hiatus in launches: data must still be analyzed, scientific questions must still be addressed, and future missions must continue to be planned. Accordingly, it is crucial that NASA examine its present support of all elements of the space program to see whether additional opportunities can be made available to sustain the program's ability to ensure its future during this difficult period.

In this Chapter we examine the implications of these requirements and of the changes identified in Chapter 2 on the issues which must be considered and the way in which NASA's Space and Earth Science Program must be conducted and managed. While there are clearly matters requiring special consideration in the short term, it must be recognized that the issues we deal with here are central to the long-term vitality of the program as well.

The Need for a Spectrum in the Scale of Flight Opportunities

To be truly vigorous, our nation's Space and Earth Science Program must support both small and large missions. The first are crucial for specialized studies, to elucidate novel ideas, and to test equipment in flight; the latter are employed to gather comprehensive data bases to be used by large segments of the scientific community in order to address the overarching scientific questions. Since the goals and contributions of large and small missions are different, science is not best served by exclusive emphasis on major missions to the exclusion of small missions. Similarly, exclusively launching suborbital flights sets the stage for, but cannot replace, the major scientific advances which can only be realized through the large flight missions. Attention must be paid to the balance between small-scale experimental missions with limited goals and the large, high-visibility projects. It must be recognized that focussed projects, research, and data interpretation are as indispensable for the successful science endeavor as are those major projects that more readily catch the public's attention.

While there are no hard and fast definitions of the various scales of space flight investigations discussed here, it appears useful to give some indication of the range of costs associated with various mission types. Individuals in the science community appear to have developed general, albeit often vague, personal operational definitions of mission types, usually in terms of dollar levels required for implementation through launch plus one month or so. Launch and operations costs are not

included in the definitions. Consequently, there is often something of an overlap in most such personal operational definitions. With these caveats in mind, low cost and suborbital projects normally are viewed as requiring a few hundred thousand to a few million dollars. Moderate-size missions occupy several ranges. For the traditional Explorers or Explorer-class missions, used by the Earth sciences, solarterrestrial, and astrophysical communities, the cost can range from \$25 million to about \$130 million dollars. A series of Planetary Observers are planned to be developed within a funding envelope of \$65 million to \$75 million per year with individual missions ranging from \$150 million to \$300 million. Development costs for the planned intermediate class Planetary Mariner-Mark II missions are estimated to be in the range of \$400 million to \$600 million. Major facility-class missions can cost from \$600 million to well over a billion dollars.

(a) Low-cost and suborbital projects.

The continued availability of frequent, relatively low-cost opportunities for carrying out observations and experiments by means such as aircraft, balloons, sounding rockets, and Shuttlecarried Spartans, Getaway Special (GAS) cans, and Hitchhikers is essential to supplement and complement the major programs described later in this chapter. Such modest-scale missions can provide significant scientific return in their own right. They are also crucial for instrument and technology development. Ideally with small missions, relatively brief periods occur between when questions are posed and when

they are answered; typically, only a few years will elapse between an idea's conception and an instrument's flight. Low cost and relatively fast turnaround make acceptable the risks associated with innovative experiments which may not be fully successful on the first try; instruments can be redesigned and reflown successfully at relatively little additional cost. Experience has shown that such frequent and rapid flight opportunities have been invaluable because they serve as essential testbeds for new instrumentation to be used later aboard major missions; in particular they have permitted the development, testing, and refinement of instrument concepts before such approaches are committed for use on extended, costly flights. Although small-scale missions may be easily justified on the basis of their technological contributions alone, we emphasize that they also provide valuable scientific data leading to both new discoveries and the acquisition of vital preliminary information for planning larger mis-

In connection with the role of low cost frequent flights, it is worthwhile to recall Freeman Dyson's recent contention that certain aspects of space science have suffered from too much planning. As he put it, "Quick is beautiful." If a discipline ever loses its ability to respond rapidly and simply to new questions, a new problem arises: its missions can end up flying out-of-date equipment to test paradigms that are equally obsolete. Rapid, elegant response is imperative: the ability to carry out high-risk missions that have the potential for overturning prevailing concepts as well as to test innovative research schemes that may fail are important elements of a vital research program.

The availability of low-cost, fast turn-around flight opportunities also plays an important role in the education process; such opportunities often allow graduate students to become familiar with all aspects of a space mission, including the design, construction, and calibration of instruments, flight operations, and data analysis. As missions have become more complex and flight opportunities have become less frequent, students have frequently been able to participate in only limited parts of a mission. As a result, their training has become less complete and thorough.

As we have noted in Chapter 2, the time scale for many major experiments, from conception and design to flight, has grown to as much as ten to fifteen years, two to three times the typical duration of graduate study; meanwhile flights have become less frequent. If those are the only kinds of flight opportunities in the program, a student may be involved in part of a flight experiment but is unlikely to see a single major project through as part of the dissertation; instead the graduate career may be spent analyzing data from a mission designed and flown while the student was in high school. While occasionally the trend towards training in only data analysis may expand learning opportunities for graduate students, most often it limits the educational experience substantially. True, some students prefer to work with data that have already been amassed by previous flight experiments, but others prefer to develop their own hardware and then utilize it to answer innovative auestions.

If students are to be educated properly and continue to be attracted to space research, appropriate graduate

experience must be provided. Otherwise, universities may graduate an entire generation of students who are not trained in the design, even to the "breadboard" stage, of instrumentation for flight experiments. It is from the ranks of these graduate students that the creators, designers, and builders of major spaceflight hardware have come and will also come in the future. A decade or more ago, graduate students in the Space and Earth Sciences were educated, as well as excited, by the opportunity to participate completely in a mission. Many of the best of today's students no longer believe that such opportunities exist. Since we have argued that a perceived future is a necessary part of a vital science, there is a danger that the best and brightest of the students will turn to other fields where there are perceived opportunities. If talented and imaginative students are not attracted to space research, its long-term vitality is in jeopardy. As we have stated in Chapter 3, without the presence of creative and dedicated people there is no program.

The scientific community is concerned that despite the importance of modest programs, research of this scale is frequently at a disadvantage in the competition for NASA funds. Ironically, such programs, although cost effective. suffer precisely because their limited price gives them a relatively low profile in the budget process. The key role of these activities in the total OSSA program must be understood. In years of severely constrained budgets, it is essential that NASA maintain a careful balance between the funding of large missions and of the smaller, less visible ones.

These low-cost programs are likely to be particularly valuable during the

current period when it is clear that new larger missions will only be started infrequently and when the NASA budget is seriously strained. As to the future, even when the launches of current major programs which have been delayed two years or more are resumed, the strengths of low cost missions indicate that such missions will always be an important component of a vital science program. While there are now serious questions as to what role the Shuttle can play in providing such flight opportunities, as we look toward the Space Station, we must ask what portion of these programs will be able to take advantage of the Space Station accommodations and what portion needs to continue as suborbital flight programs. Until it is clear that small-scale opportunities will be available on the Space Station, existing opportunities must be continued.

(b) Moderate-Size Missions.

A number of important scientific problems can be addressed with spacecraft of modest complexity and moderate cost, such as the Explorer satellites and the Planetary Observers. Such missions often follow survey or reconnaissance missions where the data indicate fundamental processes which are poorly understood and which can be studied by a limited mission. Typically, such missions are centered on a restricted set of scientific questions that can be answered by a specialized complement of instruments. Since the annual development costs of any particular modest mission are not large, relatively speaking, the presence of such missions in the total program generally allows a discipline to have significant thrusts underway in several scientific areas

simultaneously. In addition, by their very nature, moderate-size missions simultaneously furnish many of the benefits of both small and large missions. In principle at least, major scientific advances can be made on a relatively short time scale, albeit one which is clearly significantly longer than the time scale associated with the low-cost opportunities.

As the Explorer program has demonstrated to date, overall program stability can be achieved in a program of modest missions by phasing missions so that the sum total of their annual expenditures remains approximately constant, thereby providing a continuing source of new data. Clearly, any standardization which can be introduced into these continuing programs offers the prospect of reducing the total costs of each mission and hence increasing the flight opportunities. In the case of Planetary Observers, standardization is being implemented by the use of a line of similar spacecraft based on existing commercial designs, an approach recommended by the Solar System Exploration Committee (Part I). In the case of Explorers, cost reductions may also be possible through the introduction of reusable, multiple-mission spacecraft as standard platforms for those missions whose science can be accomplished in near-Earth orbit.

(c) Facility-Class Missions.

As has been noted in Chapter 2, as the result of the natural evolution in the various scientific disciplines, most of the space science disciplines have now matured to the point that they require large, long-lived, facility-class missions to accomplish major portions of their objectives. Such facil-

ities are needed because many of the most important current science questions can be answered only by obtaining measurements with increased sensitivity, improved spectral or spatial resolution, more global coverage, the use of multiple instruments or multiple spacecraft, and very long duration observations. Some of these comprehensive facilities will look down at the Earth or at other planets, while others will observe the more distant celestial objects; some will measure in situ a planet's environment, including Earth's, or even sample a planet's surface. Such facilities (for example, the Upper Atmospheric Research Satellite, the Hubble Space Telescope, the Advanced X-ray Astrophysics Facility, the Earth Observing System, Galileo, the Mars Sample Return, and so on) generally will have multipurpose instrument complements which are very versatile. Their capabilities will greatly exceed those of previous missions; moreover, they will provide huge increases in the quality and quantity of data obtained compared to previous missions or short duration experiments. These missions are expensive but they are also central to making major advances in the various scientific disciplines. Frequently the prevailing scientific paradigms can be overturned and fresh insight developed only after the stimulus provided by a burst of information from a major gain in capability.

Such missions, however, in addition to their substantial development costs, also involve a long-term commitment to sustaining costs. The consequences of assuming such long-term commitments must be carefully considered and understood because the overhead required to maintain such facilities over extended times could, unless care is taken, severely impact

other planned research. To deal with this problem, NASA may soon have to explicitly decide whether every science discipline can support such facilities. If NASA were to have to live with a fixed budget for an extended period of time, it may well find itself forced into the uncomfortable position of having to decide between providing the continuing support for forefront facilities, terminating productive ongoing smaller scale programs (a situation that could undermine the health of the program), or cancelling plans for getting into new areas of research. The situation is complex because in many cases new areas of research could be extremely fertile. In addition, the same disciplines that NASA now supports may well attract an even wider research community in the future. The power of space-based techniques appears so promising that these facilities are likely to become central to the research of many oceanographers, meteorologists, astronomers, and so on, who are not presently working under the aegis of NASA, thereby producing additional demands on the NASA program. As the result of all of these pressures NASA may face a series of almost impossible decisions involving extremely painful choices. We shall return to this issue in Chapter 8.

(d) The Role of Shuttle/Spacelab in the Space and Earth Science Program: A Post-Challenger Reassessment.

The original concept of Shuttle-based and astronaut-assisted experiments emphasized the use of Spacelab, aboard which a number of worthy scientific experiments have been flown. While the Spacelab program had not generally fulfilled its early promise of

inexpensive, frequent flight opportunities, prior to the Challenger disaster it looked as though Spacelab might soon satisfy at least some of the expectations: progress was being made to alleviate the flight delays, scientists were hoping that costs might be decreased as Shuttle flights became more routine, and many new and important experiments were being developed for future flights. The development of the Spartan and Hitchhiker capabilities offered the promise of relatively frequent access to space for low cost experiments which were substantial extensions of the suborbital program. This situation changed abruptly with the loss of one-fourth of the Shuttle fleet and the consequent grounding of the rest. Besides the obvious reality that no missions are to fly for an extended period, many associated problems are now evident.

It is clear that, as the result of the Challenger accident, access to the Shuttle for scientific use is likely to be extremely limited for an extended period of time. There is a large backlog of NASA payloads, many of which have been uniquely designed for the Shuttle. There is an equally large backup of Department of Defense payloads, many of which-because of national security considerations-will be given priority for launch when the Shuttle resumes operations. New safety and operational constraints are being placed on the Shuttle. It is likely that the launch rate following the resumption of Shuttle operations will be lower than current forecasts, even when the Challenger's replacement is available. The demands on Shuttle availability appear to be so severe that it is unrealistic to expect that more than the equivalent of a few cargo bays per year will be devoted to Spacelab, Shuttle-attached, and Spartan or other modest payloads. Thus, the expectations that the Shuttle would provide frequent, easy, and inexpensive access to space are not likely to be realized.

Even though Spacelab flights are likely to be infrequent over the next decade, there are persuasive reasons to continue to fly them. They provide unique capabilities for some kinds of science. Certain disciplines, such as life sciences and microgravity research, require manned operations and interactions. Accordingly it could be argued that the limited available Spacelab opportunities should be devoted largely to these disciplines so that these fields can remain healthy. However, if this course is pursued, the serious crowding of the Shuttle manifest means that many other science disciplines will have even fewer opportunities for access to Shuttle and Spacelab. Because of all of these evident limitations, we conclude that a major reassessment of the role of Shuttle/Spacelab in the Space and Earth Science Program is urgently needed. If the Shuttle cannot be used frequently for low or moderate cost payloads, alternatives must be made available.

Infrequent access to Shuttle and Spacelab will also affect preparations for the use of the Space Station. It was becoming increasingly clear that the experience, both bad and good, during short duration Spacelab flights, was providing valuable exposure to scientific operations which could be applied to the use of long duration shared facilities aboard Space Station. However, if the Spacelab program is extensively reduced and stretched out, at least some part of the background experience necessary to design and get ready to operate scientific experiments for

the Space Station will be missing. At present, it is not clear what impact this will have on preparations for Space Station utilization.

(e) Looking Towards the Space Station.

Whatever reservations it may have, the scientific community must recognize that the United States is committed to the construction of a Space Station and that, if intelligently planned and used, the Space Station offers the prospect of a major new capability for carrying out space research. The SESAC Task Force on the Scientific Uses of Space Station ("Space Station Summer Study Reports"), as well as OSSA management, have had a substantial influence on configuring the design of the Space Station and its associated platforms to be most useful to the entire scientific community.

However, from a scientific perspective, the Space Station is not an end in itself but only one of a set of tools to be used for addressing a broad array of scientific problems. The payloads and research activities on the Space Station must be selected according to scientific need and not just because the Station is there and must be filled up. From a scientific perspective, OSSA must not be put in the position of having to tailor its program around this single type of flight opportunity. Unfortunately, if funds remain limited as the Station continues to progress, strong pressures may develop to pursue just such a course.

The Space Station is expected to become a long-lived international research center in space. Once assembled, it will be available for an extended period, and thoughtful preparations

must be made for its use, which should start with simple experiments that evolve towards more complex ones as the Station's capabilities are better understood. Care must be taken not to repeat the mistakes of the Spacelab program in which premature commitments were made to the development of complex missions and major facilities before the program schedule was certain and while the Shuttle/Spacelab characteristics themselves were still evolving. Simple experiments should be flown aboard the early Space Station. The more complicated experiments, the one of a kind facilities, which are to reside ultimately at the Station, should only be selected, developed, and flown after the results of the simpler experiments are understood. In a similar vein, attempts to utilize completely the anticipated full capabilities of the Station from the first day should be resisted. At the outset the Station should have empty space aboard to which new experiments can be added later. Once experience is gained, there will be time enough to select a more ambitious instrument complement that makes more demands on the Station. The ease of scientific operations will also depend upon the gathering of appropriate experience, which is another reason for beginning with simple experiments. Evolution rather than revolution will be the key to successful utilization of the Space Station for science. In this way the new capabilities introduced by the Space Station can be judged along with other approaches, such as suborbital flights, Spacelab, and free flyers, and used as needed for space research.

In summary, new types of long-duration scientific experiments that

require human operation, as well as those that need periodic human inspection or servicing, will become possible with the Space Station. But the appropriate instruments that use the Station's unique capabilities should be developed only as the Station's true capabilities are learned and its schedule understood. It is imperative that the scientific community continue to have the ability to attack a broad spectrum of excellent science questions using the full range of available tools rather than restrict itself to experiments that will utilize solely the Space Station. To reiterate and generalize an earlier point, NASA must guard against the belief that operating the Space Station itself (and the Shuttle), in and of themselves, are NASA objectives, rather than ways to accomplish more basic scientific goals.

The Needed Spectrum of Flight Opportunities is Discipline Dependent

In the previous section, we have developed the case that a spectrum of scales of flight opportunities is needed to provide the continuing flow of observations and experiments required for scientific vitality. It is important to realize, however, that the needed distribution of flight opportunities differs from discipline to discipline.

Solar System exploration probably uses the fewest classes of missions. Valuable discoveries and insights have been provided by ground-based and airborne telescopes, and additional important information will come from the Hubble Space Telescope and other planned Earth-orbital observatories. Nevertheless, the primary objectives of planetary science are best addressed

if the target planetary body is closely approached so as to improve resolution and signal quality, to permit in situ measurements, and perhaps to collect samples. Hence, comets, asteroids, planets, and their moons, and ring systems are best explored by moderate and facility-class missions that leave Earth orbit and rendezvous with targets in space. These missions require special launch capabilities and propulsion systems in order to navigate across the solar system. Major engineering challenges must frequently be met and the widespread use of intricate gravityassisted trajectories for outer solar system missions often requires unusual amounts of planning.

Planetary missions such as the Mars Observer fall into the category of moderate-class missions. In defining a program strategy, the Solar System Exploration Committee (SSEC) also identified a more ambitious (and expensive) set of Intermediate Class missions such as Magellan (previously known as the Venus Radar Mapper Mission) together with a series of missions to be flown aboard a more capable Mariner-Mark II spacecraft. Examples of Mariner-Mark II missions include the Comet Rendezvous/Asteroid Flyby mission, a planned mission to the important primitive bodies of the solar system, and the Saturn Orbiter/Titan Probe (Cassini) mission. A further description of these classes of missions is given in Part I of the SSEC report which describes the "Core Program." Galileo, the orbiter and probe of Jupiter, originally scheduled for launch in May 1986, is an example of a major facilityclass mission awaiting flight. Other potential facility-class missions such as the Mars Sample Return, are currently

under study as part of the long-range plan outlined in the SSEC (1986) report on an "Augmented Program."

The Earth sciences community utilizes the full range of flight opportunities to study the ionosphere, atmosphere, ocean, and solid surface of our planet, and to develop a global picture of the interactions between these various components. For many years suborbital flights have proven invaluable to sample the ionosphere, auroral zones, and the atmosphere's chemical composition at high altitudes. Lowaltitude orbiting spacecraft have been employed to understand the terrestrial gravity field (Lageos) as well as to study the dynamics and chemistry of the mesosphere, and to develop remote land-sensing capabilities. Explorer missions have been used to study the interrelations between the chemistry and dynamics of the middle atmosphere. The Earth Radiation Budget Satellite, a moderate scale mission, is measuring the Earth's energy balance. Both polarorbiting and geostationary satellites have proven to be valuable platforms for meteorological measurements.

Recent developments in the Earth sciences have clearly shown the need for global mesurements. The Upper Atmospheric Research Satellite, a major multiinstrument facility, will scrutinize the trace composition of the atmosphere at high altitudes to resolve the interaction between dynamics and chemical processes that, in part, determine the Earth's environment. TOPEX/ Poseidon is a planned moderate scale free-flying satellite mission intended to measure the entire ocean's surface topography with unprecedented accuracy and thereby to permit inference of circulation dynamics. The proposed

Geopotential Research Mission, a moderate mission, can probe the Earth's deep interior by precisely measuring the global gravity field. The Earth sciences community is expected to be among the major users of the polar platforms associated with the Space Station, with the proposed Earth Observing System being a major facility to study global scale changes over an extended period of time. The future direction in studies of the Earth as a total system and the crucial role of large space facilities in these studies have been described in the Earth System Sciences Committee (ESSC) report.

The solar-terrestrial physics community uses the space environment as a laboratory to investigate plasmas, one of the fundamental states of matter, in a wide range of settings. This community has previously operated, and will continue to employ in the future, a great variety of observational platforms. These extend from ground-based and suborbital experiments that observe, for example, the Earth's aurorae, to planned future facility-class missions that will examine the complex interactions among the various components that connect the Sun to the Earth. Suborbital flights have been used for solar eclipse observations as well as for studies of the mesosphere and ionospheric D region. Explorer missions have fostered significant scientific advances in this field by, for example, studying the photochemistry of the thermosphere (Atmosphere Explorers) as well as the coupling between the ionosphere and magnetosphere (Dynamics Explorer). Moderate scale missions will be employed to describe the solar wind and interplanetary magnetic field morphology in three dimensions (Ulysses). Moderate, as well as facility-

class, missions will be used to explore fully the connection between the Sun's emissions and terrestrial magnetospheric processes with the various components of the planned International Solar Terrestrial Physics program (the European Space Agency SOHO and Cluster missions, the Japanese Geotail mission, and the NASA Global Geospace Science program). Shuttle/Spacelab, and eventually Space Station and its Platforms, offer opportunities to fly new types of instruments to carry out novel in situ and active experiments, often using human participation. The flight opportunities needed to address the major scientific questions in the field are described in the 1985 report of the Committee on Solar and Space Physics of the Space Science Board.

Astrophysics, like solar-terrestrial physics and Earth Sciences, utilizes the full spectrum of flight opportunities. Balloons and rockets transport infrared, ultraviolet, and x-ray instruments for short duration experiments which frequently involve the testing of new technology; balloons also are used for cosmic ray studies. NASA aircraft provide dry and clean high-altitude outposts for infrared measurements. Explorer missions (such as the Infrared Astronomical Satellite) are used for surveys or for special-purpose missions which may either lay the groundwork for facilities or carry out complementary studies.

The Great Observatories (the Hubble Space Telescope, the Gamma Ray Observatory, the Advanced X-ray Astrophysics Facility, and the Space Infrared Telescope Facility) constitute the planned centerpieces of NASA's astrophysics program. These facility-class missions will provide extraordinary long-term capabilities for detailed

studies of the cosmos. Space Station should be invaluable as a service and refurbishment center for the Great Observatories as well as providing the capability for assembling future large missions such as the Large Deployable Reflector. The NAS/NRC 1982 report, "Astronomy and Astrophysics for the 1980's", remains the primary statement of the goals of space astrophysics and describes the flight missions necessary for achieving these goals.

Life sciences and microgravity science research have not had a long history of space experimentation but are now emerging as potential major users of space for science. While these disciplines lie outside SESAC's purview, their growing needs for space missions and flight opportunities require that they be included in any discussion of the scientific utilization of space. Experiments in the life sciences and in materials research using the microgravity environment of space will usually be operated by humans; accordingly, they have been, and will be, performed principally on the Shuttle/ Spacelab, and are expected to form a major part of the Space Station. Some of the research needed to prepare for these opportunities can be done on the ground and occasionally using suborbital flights. In general, however, more extended exposures to the microgravity environment are needed. Because of the unique ties of these disciplines to the manned space program, particular care will have to be taken in the design of the Space Station to accommodate their experimental requirements if the promise of these fields is to be realized (For further discussion, see the "Space Station Summer Study Reports.")

The Need for Assured Access to Space

Without assured access to space, NASA does not have a Space and Earth Science Program.

The year 1986 was to have been NASA's widely advertised "Year for Space Science," to be marked by the Voyager Uranus encounter, the planned launch of the Astro-Halley Shuttle mission to make ultraviolet telescopic observations of Halley's comet, the launch of the Galileo and Ulysses missions, the launch of the Hubble Space Telescope, and the Galileo encounter with the Asteroid 29 Amphitrite. As the result of the Challenger accident, the subsequent grounding of the Shuttle fleet, and the cancellation of the Shuttle/Centaur program, the only success was Voyager, launched in 1977 using a Titan-Centaur expendable launch vehicle. The rest of the program has stopped and awaits the resumption of Shuttle flights. At this moment it is not clear how the Galileo mission will be launched. Other than missions already operating, the only sources of new data for the next several years will come from suborbital flights, dramatically illustrating the importance of these modest programs and the critical need for having both a diverse range of research opportunities and diverse means of launching them.

NASA should not rely on the Shuttle as its sole means of access to space. The Department of Defense recognized the need for alternatives when it began its Complementary Expendable Launch Vehicle program. The wisdom of that course is now clear. The recent Presidential policy decision directing that

the Shuttle no longer be used for the launch of commercial satellites is another step toward the diversification of the country's launch capability. NASA's scientific activities should also be able to take advantage of that diversity. While many types of scientific programs require using the Shuttle's manned capabilities, routine satellite launches do not. The most appropriate launch vehicle must be adopted for each program. The use of a mixed launch fleet will allow humans to fly when they are needed on a mission and allow unmanned vehicles to be the carrier of choice for other missions. In particular, launches having narrow time-critical launch windows can be divorced from the extra complexity and rigorous safety requirements associated with manned flights. Diversity will also allow a better matching of the scientific requirements of a mission with the launch capability needed to meet those requirements, rather than forcing the mission to meet the constraints of a single inflexible launch system.

There are many important motivations for the presence of humans in space other than the pursuit of science. The OSSA program should be able to take advantage of their presence when they are required for carrying out a particular job and to use alternatives when they are not. As in the case of Space Station, the Shuttle should be *only one of a range of tools* to be used for Space and Earth Science missions.

Research and Analysis is the Foundation for the Flight Programs

Thus far this Chapter has dealt with the means to ensure the continuing flow of observations required for

the vitality of NASA's Space and Earth Science Program. However, as we have argued in Chapter 3, the flight programs are only a part (albeit the most visible part) of the total effort required to realize the scientific return from the missions. As stated in the introduction to this Chapter, flight programs can only be successful if they are built upon a strong research base which forms the foundation of the nation's space activities.

Research, in particular theory and modeling, is required to interpret the results from space missions. Measurements and images must be tested and understood against prevailing models before they have any lasting value. If the prevailing models fail, new concepts and models must be developed and tested against the data. Frequently, extensive laboratory work must be undertaken as part of the effort of developing models. Such modeling and experimental work requires the availability of up-to-date laboratory facilities and instrumentation, as well as computer equipment. If missions are to be properly planned, then new technology must be developed in order to pursue the stimulating scientific questions revealed by the results of previous missions. Technology is advancing rapidly, and thus it is often necessary to pursue alternative approaches to the development of instruments needed to meet scientific requirements.

In July 1984, the Space and Earth Science Advisory Committee issued a report entitled "Research and Analysis in the Space and Earth Sciences." The conclusion of that report was: Endeavors in Research and Analysis form the foundation for the entire Space and Earth Science Program because they provide the means of identifying which missions are required and also the means of extracting full scientific results from completed missions. The scientific accomplishments are impressive; current objectives are well-focused and are of scientific significance.

The report also stated:

OSSA must give its Research and Analysis (R&A) Program a priority in funding and attention commensurate with that of flight programs. The endeavors of R&A are the foundation for the entire Space and Earth Science Program. The success of OSSA in contributing to the NASA mission has been and will be determined by the successes of the R&A Program.

We reaffirm the conclusions of that report. Unfortunately, since that report was issued there has been a continued erosion in the funding for these critical activities, primarily as the result of the deficit reduction climate. Research and Analysis is not a luxury. It is a central element in a vital Space and Earth Science Program. It must be strengthened and protected from funding fluctuations.

Summary

This Chapter has pointed out that a vital Space and Earth Science Program requires a broad spectrum of flight opportunities, the mix of which differs from discipline to discipline. Low cost flights, major missions, and basic research and analysis are equally crucial for producing excellent science. The Shuttle and manned flight continue to have pivotal roles in Space and Earth Science, but expendable launch vehicles must also be reintroduced into the nation's space fleet for many future missions. The precise blend of expendable launch vehicles and Shuttles that will allow the best scientific program must be chosen carefully. Future Shuttle/Spacelab flights are likely to be infrequent and a major reassessment of the role of Shuttle/Spacelab in the Space and Earth Science Program is needed. Preparations for use of the Space Station should start with simple experiments which evolve towards more complex ones as the Station's capabilities are better understood. Research and Analysis must be given a priority in funding and attention commensurate with that of flight programs.

With the hiatus of Shuttle flights, and with access to the Shuttle likely to be limited once flights are resumed, many of the assumptions on which the NASA Space and Earth Science Program had been planned are no longer entirely valid. As the program is being replanned, great care must be taken to provide the diverse range of activities needed to ensure its long-term health and vitality.



Chapter 5:

Requirements of a Vital Space and Earth Science Program — The Need for People and Institutions

As argued in Chapter 3, there cannot be a vital Space and Earth Science Program without the presence of talented and dedicated people, which in turn requires the existence of the appropriate institutional framework in which such people can exercise their talents. NASA necessarily occupies the central position in the national Space and Earth Science research enterprise and is a major factor, as well, in the international Space and Earth Science arena. Organizing and conducting research over the range of disciplines encompassed by the Space and Earth Science Program is an extremely complex undertaking and requires a variety of skills and capabilities as well as extensive facilities and the appropriate institutional framework. It was not expected, when NASA was established, that it would do the job alone.

The space research community involves a number of key institutional components, domestic and international. Although the non-NASA components are influenced to a greater or lesser extent by NASA, they are, in fact, autonomous organizations whose goals overlap only in part with those of NASA. Thus, the total Space and Earth Science enterprise is ultimately connected by the relationships among institutions that are established in pursuing common objectives with NASA. It is thus necessary for a successful program that all institutional components, including

NASA, understand the respective goals and aspirations of their various partners in the research endeavor.

This Chapter examines the composition of the total space research community, describes the unique contributions each component makes to space research, and characterizes the interrelationships among the various organizations. Suggestions are made as to how best to use these diverse institutions to carry out the most effective Space and Earth Science Program in the U.S. We conclude that the different organizations bring different and complementary capabilities to researchcapabilities which are all needed to maintain an effective Space and Earth Science Program.

The Component Institutions of the Space and Earth Science Infrastructure

The focal point for planning and administering the U.S. Space and Earth Science Program is NASA Headquarters in Washington, D.C. While NASA Headquarters provides leadership and direction to the program, numerous other segments of society have varying levels of influence, including final budgetary authority. Members of the scientific community make their views known

both collectively, through such organizations as the NASA advisory committees and the National Academy of Sciences and its committees, and individually, by frequent contact with NASA Headquarters personnel, other Federal agencies, the Office of Science and Technology Policy, Congress, and so on. The end of the chain is the budget process, wherein the Executive Branch proposes, and the Legislative Branch authorizes and appropriates spending of Federal revenues.

While NASA Headquarters directs the overall Agency program, the scientists and engineers who actually perform the work are located at the NASA field centers, at universities, in industry, and frequently within other Government agencies. A significant number of foreign scientists and institutions also contribute in various ways to the U.S. Space and Earth Science Program. The diverse roles and the interactions among these participating institutions are discussed below.

(a) NASA Centers

The NASA Centers play several important roles in the conduct of space flight projects. First, NASA centers manage NASA space projects. They have the capabilities to carry out the complex and highly specialized tasks involved in the execution of space projects such as project management, quality assurance, testing, integration, and launching. These tasks are often too large in scale to be readily carried out as part of the educational objectives of a university.

Second, NASA centers perform space-related research and development. Some of this research is unique to NASA centers, while some of it is in direct competition with similar research performed at universities and other types of laboratories. Recently there appears to be a trend toward increased cooperation between center and university research groups, with the centers often providing facilities such as large-scale computers, research aircraft, or calibration and testing facilities which are not routinely available elsewhere. University researchers can also visit NASA centers for extended periods of time through the Resident Research Associateship and the Summer Faculty Fellowship Programs.

The purposes of the NASA in-house research programs are two-fold: (1) to perform research and development which is essential to NASA's programs, much of which is not, or cannot be, done by other institutions, and (2) to give the centers the cadre of competent scientists required to understand the scientific requirements and to ensure the scientific integrity of flight projects.

(b) Universities

Universities educate the new scientists and engineers who will participate in all aspects of NASA science and technology. Universities are the sole institutions whose primary responsibility is to ensure a supply of talent. The success of the educational enterprise in the Space and Earth Sciences depends upon the nature of the interaction between NASA and the universities. The role of NASA flight projects in the education of new scientists was discussed in Chapter 4. An important strength of the universities is their constant flux of people, especially students, into and out of an environment where ideas and knowledge are the principal commodity. It is at universities that

most students are introduced to the Space and Earth Sciences. From these students must come the highly-trained industry, university, and government personnel who must be available if our future as a spacefaring nation is to be assured

Universities, by their nature, have a particular responsibility to collect and maintain the accumulated knowledge of civilization across all areas of learning. This knowledge includes the Space and Earth Sciences. Discovery, synthesis, and transmission of knowledge are the prime functions of a university. The intellectual environment of universities encourages innovation, and university scientists have broader freedom to pursue ideas than is found in many other settings. Interactions with students introduce new ideas and perspectives into the Space and Earth Sciences, as do interactions with colleagues from other disciplines. The quality of the transmission of knowledge of the Space and Earth Sciences thus depends upon the continued health of the academic community. The future health of the Space and Earth Sciences depends in great measure on the attractiveness of space research to students. As emphasized in Chapter 4, it is important to have flight opportunities which can be carried out on time scales consistent with graduate education. But, if students do not perceive a future, including the ultimate availability of research employment, their interest in the Space and Earth Sciences will fade quickly.

(c) Industry

The role of industry in the national space program is fundamental and broad, encompassing manufacturing,

large-scale engineering, design, test, construction, and the operations of space missions, as well as smaller scale research and development tasks. The major efforts are carried out by firms in the aerospace industry, while other segments of industry, primarily electronics, instrumentation, and optical companies, provide payloads and components. The essential contribution of industry is to provide the planning staffs, the large numbers of engineers, technicians, and skilled workers, and the elaborate facilities and equipment required to design and construct largescale space qualified equipment.

Major aerospace companies, together with NASA center and university personnel, often conduct studies of future mission concepts, perform feasibility evaluations of proposed NASA projects, and bring hardware design and construction experience to bear on programs at an early stage. Small businesses also contribute to the NASA program as suppliers of manpower, specialized equipment, and services to the Agency.

Private industry is also involved in the research phases of NASA science programs. Scientists in industrial laboratories are attracted to the exciting research prospects of the space program and contribute to the Nation's basic research effort. An important capability of industrial research laboratories is the ability to quickly assemble a well-qualified team having the diverse expertise needed for space hardware construction and testing. As the complexity of instrumentation has increased, both NASA centers and universities have come to rely increasingly on industrial firms for developing major portions of flight instruments.

(d) Other Federal Agencies

A number of other Federal agencies, in addition to NASA, are actively engaged in Space and Earth Science research. In some cases, these agencies have specific responsibilities mandated by the Government to provide services or to maintain research activities that are closely related to NASA's goals. Some of the space-related activities of these other agencies are listed in Table 1.

The breadth and depth of space research is often enriched by the involvement of multiple agencies. Different agencies contribute differing viewpoints and unique expertise which enhance the NASA program, even though their space research activities are necessarily oriented toward their own standards and missions. The pursuit by these other agencies of applications derived from space research reinforces and complements the programs conducted by NASA.

International Cooperation

The emergence in other countries of a substantial capability for conducting out major missions in the Space and Earth Sciences, at a time when U.S. resources are strained, introduces a new factor into considerations of the vitality of the U.S. space community. There are now important prospects for cooperation and competition. In order to deal with this rising capability today at the mission planning level. coordination with non-U.S. missions is a necessity, both to avoid duplication and to take advantage of the complementary nature of many projects. The ability of both U.S. and non-U.S. scientists to compete for experimental opportunities on both U.S. and nonU.S. missions can produce the best scientific results.

Valuable, but probably unmeasurable, benefits also arise from the close personal ties and working relationships among scientists which have developed from cooperative programs. A number of highly successful international programs involving major sharing of space hardware (such as the International Ultraviolet Explorer, the International Sun-Earth Explorers, the Infrared Astronomical Satellite, and the Active Magnetospheric Particle Tracer Explorer) have been conducted. Significant contributions have also been made by the inclusion of non-U.S. instruments on U.S. space missions. Even broader international involvement has been realized by the non-U.S. co-investigators who have been members of teams led by U.S. principal investigators. While the overall impact of international cooperation has been beneficial, there have been a few unfortunate cases which have worked out badly. In the most notable example, the abrupt cancellation of the U.S. spacecraft component of the International Solar Polar Mission (now called Ulysses) disillusioned our European partners in that program and has produced a lingering doubt about the dependability of U.S. commitments. If international activities are to be an important part of the NASA science program, a way must be found to ensure that programs, once started, can proceed on a secure course.

Until recently, a serious concern has been that while non-U.S. scientists can compete freely for experimental opportunities on U.S. missions, there has been no reciprocal agreement enabling U.S. scientists to compete for experiments on non-U.S. missions.

Table 1. A Sampling Of Federally-Sponsored, Space-Related Activities Outside NASA

Agency

Example Space-Related Activities

Department of Defense

• Air Force Geophysics

Laboratory

Natural Background Radiation

Space "Weather" Geomagnetism Solar Activity

Upper Atmosphere

Office of Naval Research

Broad Range Of Research In Space

And Ocean Sciences

Naval Research Laboratory

Solar Physics

Ultraviolet Astronomy Space Plasma Physics

National Oceanographic and

Atmospheric Administration

Weather

Oceanography
Space "Weather"

National Science Foundation

Plasma Physics

Atmospheric Chemistry
Earth and Ocean Sciences

Department of Energy

Space Environment

Climatology

While some non-U.S. missions have carried U.S. experiments, these cases have been the exception rather than the rule. True reciprocity must be implemented to safeguard the respective interests of both the U.S. and the non-U.S. scientific communities. The European Space Agency (ESA) has agreed to a policy of reciprocity on scientific missions whereby U.S. scientists are invited to propose for ESA missions. Valuable opportunities have also been offered for participation of U.S. scientists on Japanese missions. These trends should be strongly encouraged.

Continued, and even enhanced, international cooperation, including true reciprocity on bilateral and multilateral bases, can enhance the space research capabilities of all participants, and may play a valuable role in ensuring the long-term vitality of the U.S. space research community.

Changing Roles of Institutions in the Space Program

As discussed in Chapter 2, one of the most notable developments stimulated by the increased complexity of space instrumentation has been the formation of consortia in which several research groups participate in the design and construction of instrumentation that is beyond the capabilities of a single institution. Building modern, space-qualified experiments is now an exacting discipline requiring large fixed assets, specialized facilities, and substantial management and engineering staffs. As a result of these requirements, fewer institutions are now able to carry out a complete space research project without multiinstitutional collaboration.

Developments in associated technology are also changing the nature of

the interactions among the institutional elements of the space research community and providing new opportunities. For example, improvements in communications and the ability to transfer data between institutions make collaboration possible on a worldwide basis. Individuals can work together on a program almost irrespective of their affiliation and geographic location; thus scientists can be active in space research and make specialized contributions to a larger program even if their parent organizations do not have the capability to carry out all aspects of a major experiment program.

Concern has been voiced that the emergence of space research as "big science," discussed in Chapter 2, and the increasing use of consortia have led to a decrease in participation in space research by universities. In fact, a close examination of the situation indicates that the proportion of participation by the various types of institutions appears to be fairly stable. In the period 1972 to the present, the affiliations of Principal Investigators on major NASA missions have averaged about 30 percent university, 30 percent NASA centers, 18 percent non-U.S., 13 percent other Federally-funded laboratories, and 8 percent industry. Although the mix of institutions varies from mission to mission, and more institutions may be involved in a single experiment, there is no evidence that the institutional mix of leadership roles has changed appreciably since 1972.

Conclusions

It is apparent that the institutional framework supporting and participating in U.S. space research contains several distinct types of organizations

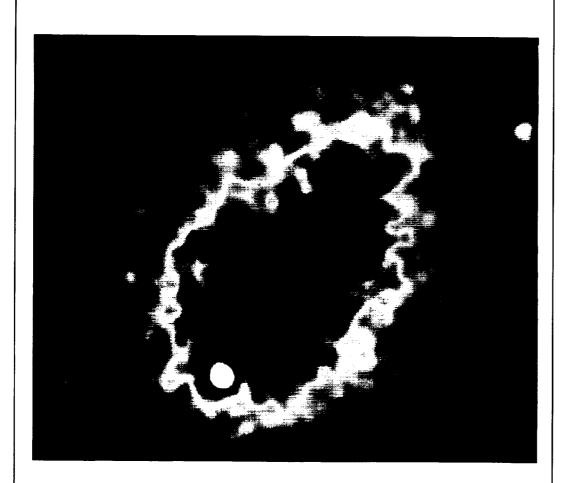
with various interests, motivations, and capabilities. This *diversity* is a major strength for the research, but for maximum effectiveness the overall program must be properly orchestrated so that the strengths of each participating institution can be used to best advantage. Thus, the work of the various institutions participating in space activities must be carefully coordinated to promote the overall national effort.

The needed coordination among the major institutional entities participating in the NASA program must involve other Federal agencies and the non-U.S. space programs as well. An increase in the number of cooperative ventures, including joint flight projects, is beneficial for the science. Such cooperation could well lead to increased frequency of access to space, as well as to a broader, more diverse base for Space and Earth Science research endeavors.

To maximize the return from the total U.S. investment in space research, the independent or separate functions and programs of the various participating U.S. institutions and agencies must be carefully managed to avoid duplication and increase opportunities. It should be recognized that, in some cases, proper coordination could, and perhaps should, lead to shifts in responsibility from one institution or agency to another. A possible example would be the planning of a mission for which the operational (as opposed to the development) responsibilities for a

scientific satellite would ultimately be transferred in an orderly fashion from NASA to another, more appropriate, agency such as the National Science Foundation. Such a step would be a significant departure from past practice and should be implemented only after careful preparation. Formation of more partnerships among the various sectors could also increase the efficiency of the Space and Earth Science Program. Collaboration of university scientists with colleagues and resources available at national laboratories or in industry seems a particularly effective combination, as do certain NASA-other agency and U.S.-foreign collaborations.

The stresses on the Space and Earth Science Program, outlined in Chapter 2, have been passed on to the organizations which participate in space research. As noted there, the Challenger accident has transformed a severely stressed community into one in a state of crisis. At this time, it is imperative to recognize the need to keep the scientific community active and productive, and to take action to instill a sense of future promise into the community. As emphasized in Chapter 3, a vital science requires a perceived future. If the infrastructure is allowed to deteriorate during the current emergency, any future restoration of the community will be slow and expensive. Without talented people and the institutions to support them, there will be no Space and Earth Science Program.



Chapter 6:

The Difficult Decisions

The NASA program in the Space and Earth Sciences is a complex mosaic of efforts that involves a broad range of activities reflecting a continuously evolving set of interacting priorities. As the program has grown in scope, technical complexity, and cost it has become increasingly essential to develop means to formalize the establishment of priorities. Charting a course through these difficult times requires an examination of the decision-making process associated with the Space and Earth Science Program. In this Chapter we attempt to identify the many issues that must be considered in order to reach effective decisions and we attempt to provide a foundation for use at all decisionmaking levels in formalizing and rationalizing the process of making the choices that are crucial to the future of the NASA Space and Earth Science Program. The critical step, we believe, is to clearly define and state the criteria on which the major decisions must be based. Then it becomes possible to develop procedures for making the crucial choices.

The justification the science community provides for public support will be greatly enhanced if we can demonstrate that we have been as thorough and as rational in our scientific choices and programmatic decisions as we are in pursuing the enigmas of nature.

Competition, Criteria, and Choice

The most difficult issues in the support and management of science concern the apportionment of resources among the available opportunities.

When resources are limited or scarce, some excellent opportunities must be neglected in order to pursue those that seem to have more merit. Programs, disciplines, institutions, and indeed the future course of science itself, are affected dramatically as a result of such decisions.

The NASA Space and Earth Science Program now faces numerous difficult decisions concerning resource apportionment, in part as the consequence of its own successes. As noted in Chapter 2, even though the resources made available to OSSA have increased slightly in the last few years, the opportunities for challenging and meaningful scientific initiatives have increased much more rapidly. To compound the problem, the capabilities and costs of space science missions have increased with the development and utilization of sophisticated technologies in optics, electronics, communication, computation, and automation. In addition, missions have become more costly to operate. On top of all of this, severe stress has been placed upon the program resulting from the large extra costs associated with the delays due to the Challenger accident, thereby making even more critical the need to rationalize the decision-making process. Decisions concerning large missions involve significant near- and long-term resource commitments and affect the future course of entire fields of science.

In the process of selection of new initiatives, major missions are advocated in fairly mature form when they reach the point of competing for New Start

status. They reach this highly competitive level through a complex process: NASA receives advice and proposals from the scientific and technological communities through a variety of channels, including the committees and panels of the Space Science Board of the National Research Council, from the NASA Centers, from NASA's own internal advisory panels and committees, from its staff, and from individual scientists. A new mission concept may emerge from any of these sources, and often new missions are part of an agreedupon strategy within a particular scientific area. Those that address important scientific issues and that pose feasible technological requirements begin to attract support and to gain more specific form through scientific, technological, and feasibility studies. Many such proposals are abandoned for various reasons, but some gain both identity and momentum, finally appearing in an informal cluster of potential New Starts.

In recent years, some four or five potential major missions have competed for inclusion in an annual budget submission that was envisioned to contain, at most, two New Starts. Another ten or so proposed missions were considered to be less mature or less urgent, but remained identified as potential future New Starts. The competition has intensified dramatically as NASA's proposed New Starts were either stricken from budgets or stretched out and delayed; in turn, this meant that these same candidates were still present in the competition of succeeding years.

The increasing intensity of this competition between proponents of new missions, the serious consequences for the disciplines whose missions are delayed or rejected, and the even more intense competition that can be antici-

pated in future years combined to motivate this examination of the values and criteria that should be considered in arriving at decisions on new missions. Decisions must be made on the basis of explicit consideration of the widest possible range of factors concerning the merit of individual programs. Thus, a formal evaluation and comparison process is mandatory. Criteria are necessary and must be identified.

Two basic issues thus confront NASA in the management of its Space and Earth Science Program. The first is whether the potential scientific accomplishments and societal benefits of a particular program can be identified in advance so as to justify a commitment from NASA, and hence, national resources. The second is whether a reasoned, effective procedure can be developed to distinguish and choose among competing opportunities. If the American public wants a strong Space and Earth Science Program and recognizes its potential benefits, then NASA must demonstrate that it has a rational way for making its decisions about which Space and Earth Science initiatives deserve to be supported. Responsible evaluation of proposals requires the formulation of criteria that will illuminate the crucial aspects of the competing opportunities. At the same time, advocacy and management of the overall Space and Earth Science Program will be greatly enhanced through the thorough and systematic examination of the essential merits and potential benefits of the new programs.

Allocations of resources to major Space and Earth Science initiatives are always made in the context of national science and economic policy. The process involves consideration of each opportunity against other competing scientific and technological initiatives, as well as the possible uses of those resources for other public purposes. Proposed Space and Earth Science Programs are considered by NASA managers, acting with awareness of the recommendations of the scientific community, by other levels of the executive branch, and by Congress. Furthermore, interactions with international agencies and recognition of other nations' scientific activities and political imperatives are becoming increasingly important. Various criteria have been used with differing emphasis by the different participants in order to make decisions. While importance to progress within a science discipline and maintenance of the integrity of the infrastructure tend to dominate the considerations of the proposing research discipline, much more complex issues involving maintenance of overall scientific strength and balance and national policy considerations dominate at higher levels.

Broadly speaking, the national decisions about resource allocations to Space and Earth Science initiatives require evaluation of scientific merit, programmatic considerations, and assessment of potential societal benefits. From the perspective of the scientific community, decisions concerning the Space and Earth Science Program must be made on the basis of quality of science and must favor the most significant scientific opportunities. Ideally, programmatic implications and societal impacts would be considered as secondary. The contributions of proposed Space and Earth Science initiatives to the public welfare, in both direct and indirect ways, usually will not have an important bearing on OSSA decisions, but they are always

significant at other levels of advocacy. A fundamental and continuing issue is the relative support to be given by NASA to major individual Space and Earth Science flight missions in contrast to support for development efforts, the community, and the institutions which we have argued are necessary to maintain the vitality of science and produce new opportunities for missions. Decisions concerning these other components also have significant implications and are often not thoroughly considered.

The advice NASA receives from the science community through its formal advisory structure such as SESAC and the Space Science Board can be expected to concentrate on evaluation of the scientific significance and quality of proposed initiatives or missions and to recommend priorities among them. However, the scientific community must also be cognizant of both programmatic and societal considerations which drive the decision-making processes used by NASA and others. Consideration of the full range of criteria relevant to evaluation of scientific initiatives is thus important in consideration of priorities.

It is of the utmost importance to recognize that the process of arriving at a decision concerning major new initiatives is not one of rating or judging between the merits of competing scientific disciplines; rather it involves the evaluation of the initiatives or missions already proposed and the determination of priorities within that proposed set. Any scheme to evaluate and recommend programs must consider both criteria that are internal to a discipline and criteria that are external and that thus acquire their significance from the broader context of value to

a wider body of knowledge and to society at large (see, for example, the discussion in Weinberg, 1963). Because the proposals competing for assignment of priorities will have undergone close scientific scrutiny within the individual science disciplines, generally only a few arguments may surface in terms of comparative scientific value, and it must be recognized that at times the difficult selection decisions may then be dominated by programmatic or societal issues.

The Identificiation of Criteria

The establishment of explicit criteria as the basis for decisions will allow NASA managers to be most effective in making decisions and recommendations, in communicating with each other, and in advocating the broad program in other forums. OSSA frequently must deal with questions of balance within or between disciplines which cover a broader range of research than is formally examined by a single advisory committee. Any attempt to develop an evaluation procedure for Space and Earth Science proposals must determine which values are relevant and what relative importance should be assigned to each. Perception of the significant values must be followed by formulation of criteria that reflect these values and permit assessments or evaluations of inherent worth to be made as consistently and as unambiguously as possible, even though quite different scientific disciplines and types of investigations are being considered. Because of the wide range of significant science opportunities being considered, and because the competition among candidate missions often reflects different scientific stages and

maturities, this evaluation always is destined to be difficult.

(a) Scientific Merit

The fundamental purpose of the Space and Earth Science Program is to obtain scientific understanding of the world around us; hence, scientific merit and potential scientific contributions must be the dominant values to be assessed. However, to use scientific merit as a criterion, one must consider the nature of science in general and the nature of the Space and Earth Science Program in particular.

The discussion in Chapter 3 can be recast to state that science involves three modes of activity described as exploration and discovery, reconnaissance and observation, and theory and modeling. Recognition that the ultimate goal of a scientific investigation is to acquire sufficient information about a phenomenon to enable modeling and prediction and, therefore, increase understanding leads to an important conclusion: The work of science is not done until discovery is reinforced by observation, and until observation stimulates theoretical explanation and modeling. Therefore, each mode of scientific activity is potentially of equal merit.

Thus science proceeds in an interactive circle of activities as described in Chapter 3. At a particular time in the evolution of a particular scientific discipline, the efforts associated with discovery may be the most meritorious; at other times, those associated with observation or modeling may promise the greatest scientific benefits. Ultimately, of course, science always needs the stimulus of new discoveries: without new challenges, science tends to

become convoluted, introspective, or concerned with peripheral issues. Thus, criteria must be applied within the context of the current needs of the scientific disciplines. These considerations point toward a working definition of scientific merit:

Because the goal of science is to produce rational understanding of phenomena in physical, chemical, or biological domains, scientific enterprises are meritorious in proportion to the extent that they reveal the laws and interactions governing the structure and evolution of those phenomena. The wider the domain and the broader the scope of a scientific law or theory, the more universally applicable it is, which, frequently implies value.

As noted earlier, the task of comparing competing scientific disciplines is probably intriniscally impossible. The discussion contained in Table 2 entitled "Which is More Meritorious?" illustrates the difficulties of comparing general scientific merit between disciplines. Rather, the above definition is meant to apply as guidance, not for rating competing disciplines, but for helping determine the degree of merit associated with a specific scientific initiative.

(b) Programmatic Considerations

NASA has a broader responsibility than conducting individual space flight missions, however meritorious each might be; it also must ensure that the Space and Earth Sciences remain vigorous and focused on fundamental scientific questions, and that the range of activities undertaken constitutes a

coherent total program. Thus the programmatic implications of proposed missions must be ascertained with particular attention devoted to weighing the costs of a proposed mission against its benefits and to comparing the benefits that might accrue from pursuing other opportunities instead. In addition, as we have argued in Chapters 4 and 5, there must be a melding of all the diverse elements of Space and Earth Science research, both large and small, and the health of one aspect of the program may need to be judged against the new opportunities in another.

(c) Societal Benefits

Societal priorities must be considered as well because the OSSA budget is drawn from public funds and is a significant fraction of both the NASA budget and the total U.S. expenditures for science. The major OSSA missions are truly national efforts, and thus the establishment of priorities has implications for the nation as a whole. Although Space and Earth Science missions and initiatives are considered basic research, they also produce direct societal benefits through development of technology, through stimulation of the economy, and through promotion of international cooperation. These benefits augment the purely intellectual rewards of an improved understanding of our world and universe.

While OSSA recommendations must necessarily be centered on the scientific perspective, they will also be derived from considerations of the societal implications of each program since at other stages of the decision-making process groups and individuals involved in decisions about space research resources will examine these implications more thoroughly.

Table 2. Which is More Meritorious?

The difficulties in assessing the scientific merit between questions from disparate fields are illustrated by the following comparisons.

Which has greater scientific merit:

- Discovering the distribution of the zeros of the Riemann Zeta function or discovering the distribution of gamma ray sources in the universe?
- Discovering the causes of space sickness or discovering the factors that control the volume of ice on Mars?
- Discovering the key instability controlling evolution of severe storms or discovering a specific bifurcation path governing the transition to fluid turbulence?
- Discovering F = ma or discovering the integers and binary numbers?
- Discovering a successful parameterization for the turbulent flux of heat and moisture from the ocean surface or discovering a successful parameterization for the rate of vegetative carbon fixation as a function of a greenness index?

Detailed Criteria for Selection

The preceding discussion has emphasized that criteria for evaluating proposed missions or new initiatives must illuminate issues related to scientific merit, programmatic considerations, and societal implications. In this section, we present a detailed set of criteria, phrased as questions, designed to foster a structured evaluation of pro-

posals for Space and Earth Science initiatives or space flight missions. The questions may need to be applied somewhat differently depending on whether the particular proposal concerns exploration and discovery, reconnaissance and observation, or theory and modeling. With slight modification in wording, the criteria also appear to be suitable for analyzing other types of scientific budget and priority issues.

I. Scientific Merit

A. Scientific Objectives and Significance.

- 1) What are the key scientific issues being addressed by the mission or initiative?
- 2) How signficant are these issues in the context of science?
- 3) To what extent is the mission or initiative expected to resolve them?

B. Generality of Interest

- 1) Why is the mission or initiative important or critical to the proposing scientific discipline?
- 2) What impacts will the science accomplished by the mission or initiative have on other disciplines?
- 3) Is there a potential for closing a major gap in knowledge, either within an important discipline or in areas bridging disciplines?

C. Potential for New Discoveries and Understanding

- 1) Does the mission or initiative provide powerful new techniques for probing nature? What advances can be expected beyond previous measurements with respect to accuracy, sensitivity, comprehensiveness, and spectral or dynamic range?
- 2) Is there a potential for revealing previously unknown phenomena, processes, or interactions?

- 3) In what ways will the mission or initiative answer fundamental questions or stimulate theoretical understanding of fundamental structures or processes related to the origins and evolution of the universe, the solar system, the planet Earth, or of life on Earth?
- 4) In what ways will the mission or initiative advance understanding of important and widely-occurring natural processes and stimulate modeling and theoretical description of those processes?
- 5) Is there a potential for discovering new laws of science, new interpretations of laws, or new theories concerning fundamental processes?

D. Uniqueness

- 1) What are the special reasons for proposing this investigation as a mission in space or as an OSSA initiative? Are there other ways that the desired knowledge could be obtained?
- 2) Is there a special requirement for launching the mission or starting the initiative on a particular time schedule?

II. Programmatic Considerations

A. Feasibility and Readiness

- 1) Is the mission or initiative technologically feasible?
- 2) Are substantial new technological developments required for success?
- 3) Are there adequate plans and facilities to receive, process, analyze, store, and distribute data at the expected rate of acquisition?
- 4) Are there adequate plans and funding identified for scientific analysis of the data?
- 5) Is there an adequate management and administrative structure to develop and operate the mission or initiative and to stimulate optimum use of the results?

B. Space Operations and Infrastructure

- 1) What are the long-term requirements for space operations, including launches, replacement and maintenance of instruments, and data acquisition and transfer?
- 2) What current and long-term infrastructure is required to support the mission or initiative and the associated data processing and analysis?

C. Community Commitment and Readiness

- 1) Is there a community of outstanding scientists committed to the success of the mission or initiative?
- 2) In what ways will the community participate in the operation of the mission or initiative and in the analysis of the results?

D. Institutional Implications

- 1) In what ways will the mission or initiative stimulate research and education?
- 2) What opportunities and challenges will be presented to NASA Centers, contractors, and universities?
- 3) What will be the impact of the mission or initiative on OSSA activities? Will new elements be required? Can some current activities be curtailed if the mission or initiative is successful?

E. Collaborative Involvement by Other Agencies or Nations

- 1) Does the mission or initiative provide attractive opportunities for involving leading scientists or scientific teams from other agencies or other countries?
- 2) Are there commitments for programmatic support from other nations, agencies, or international organizations?

F. Costs of the Proposed Mission or Initiative

- 1) What are the total direct costs, by year, to the OSSA budget?
- 2) What are the total costs, by year, to the NASA budget?
- 3) What portion of the total costs of the mission or initiative will be borne by other agencies or nations?

III. Societal And Other Implications

A. Contribution to scientific awareness or improvement of the human condition

1) Are the goals of the mission or initiative related to broader public policy objectives such as human welfare, economic growth, or national security?

- 2) What is the potential for stimulating technological developments that have application beyond this particular mission or initiative?
- 3) How will the mission initiative contribute to public understanding of the physical world and appreciation of the goals and accomplishments of science?

B. Contribution to International Understanding

- 1) Will the mission or initiative contribute to international collaboration and understanding?
- 2) Do any aspects of the mission or initiative require special sensitivity to the concerns of other nations?

C. Contributions to National Pride and Prestige

- 1) How will the mission or initiative contribute to national pride in U.S. accomplishments and to the image of the United States as a scientific and technological leader?
- 2) Will the mission or initiative create public pride because of the magnitude of the challenge, the excitement of the endeavor, or the nature of the expected results?

Trial Applications of the Criteria

The criteria for making decisions that have been proposed here have been used in two applications as the basis for setting priorities. In one trial, SESAC used them to refine its procedure for arriving at priorities concerning candidate missions being considered for New Start status in Fiscal Year 1988. In another, a committee developing a long-range plan for National Science Foundation (NSF) support of atmospheric sciences used them to select and rank major initiatives within the discipline.

A crucial aspect of both applications was the development of written responses to the questions contained in the list of criteria by advocates of the proposed missions or initiatives. The availability, in uniform format, of documentation addressing the criteria greatly facilitated the comparison and evaluation of proposals.

In both applications, the use of evaluation and selection procedures based upon the proposed criteria seemed to be an important factor in allowing the groups to reach a concensus about priorities and to formulate definite recommendations.

Conclusion

The difficult decisions concerning allocation of resources to competing proposals for Space and Earth Science initiatives and missions have broad consequences, both for science and for the nation. Arriving at such decisions requires careful analysis of scientific merit, programmatic implications, and societal considerations. Because of the importance of these decisions, we have sought to formulate a suitable set of criteria for developing recommendations on resource allocations

and priorities. We have attempted to aid the decision-making process by identifying the various values that we expect science to advance, and by specifying criteria that will foster evaluation of proposed initiatives or missions. Formalizing the procedure, we believe, will lead to more effective decision making by OSSA and NASA.

Our concern with the decisionmaking process reflects the difficulty and significance of the task. Our recommendations are intended to bring forth, not supplant, the wisdom necessary to make these difficult decisions.



Chapter 7:

Managing the Space and Earth Science Program: Optimizing the Use of Resources

The previous chapters have identified the changes in the nature of the Space and Earth Science Program over the past decade or more, the requirements which must be met if NASA is to continue to have a vital scientific program, the range of activities that must be included in the program in order to satisfy those requirements, the roles of the various institutions involved in the program, and the considerations that must enter into the decisions concerning the large missions—the centerpieces of NASA's scientific endeavors. Identifying changes, stating requirements for vitality, defining roles, and setting criteria for making choices will, however, merely constitute an intellectual exercise if specific actions are not taken both by NASA management and the scientific and engineering communities to improve the effectiveness of program implementation. The precious resources—people, funds, facilities-needed to convert important scientific initiatives into reality will always be limited, and thus the utilization of these resources must be optimized if NASA and the nation are to obtain the maximum return from their investment in space science.

Advances at the frontiers of the Space and Earth Sciences do not come cheaply. For nearly three decades the American people have accepted the costs associated with scientific discovery and progress; there is every indication that this support continues today.

Nevertheless, the nation does rightly demand that it receive the most return possible from the dollars spent. Although it is evident from the discussion in Chapter 2 that the scientific possibilities exceed the available resources, neither NASA nor the scientific community can claim that all the Space and Earth Science Program requires to take advantage of emerging new opportunities is ever increasing amounts of money. Other steps must also be taken. At present the Space Science and Applications Program is receiving approximately \$1.5 billion per year. NASA's first priority must be to ensure that those resources are utilized as effectively as possible. Only when that is done can the additional future needs of the program be properly assessed. In this Chapter, we examine the current implementation of the program and discuss a number of steps which could be taken to control the costs and to optimize the use of resources in the future Space and Earth Science Program.

Control of Spacecraft and Instrument Costs

A growing burden for space research is the increasing cost of missions of all categories. Several innovative approaches should be considered in order to decrease spacecraft costs, particularly for missions that are medium to large in scale. Savings may

be obtained in the Explorer and Planetary Observer class missions by introducing more continuity and standardization into these programs. As recommended by the Solar System Exploration Committee, this strategy is being implemented for Planetary Observers by adapting existing near-Earth orbital spacecraft designs to satisfy the science requirements of diverse planetary missions. Spare parts from past missions may also be employed on future programs at considerable cost savings, although there is clearly a limit to how far this practice can be carried given the current trend towards the use of protoflight models with limited spares. Expenditures also might be reduced on Explorer missions by introducing multiple-mission spacecraft buses as standard platforms. We note that such an approach is currently being considered.

Consideration should also be given to developing instruments that could be used on several missions. If such approaches are taken, engineers will need to be unusually creative or science goals may have to be compromised somewhat; nevertheless, by reducing the costs of individual missions, such strategies might provide the additional flight opportunities which are so badly needed to ensure the future vigor of the Space and Earth Science Program. On its part, the scientific community may have to be more willing to accept compromises than was necessary in the past. A careful distinction must be made between the spacecraft and instrument capabilities required to meet the scientific objectives of a mission and the ultimate capabilities which might be technically realizable. It is important to avoid situations wherein a significant fraction of the mission cost is associated

with achieving a relatively modest increment in performance that may not really be needed. Both instruments and missions must be sized for the expected return.

Similarly, overall expenditures may be reduced by decreasing the amount of documentation and inspection required for scientific experiments that are carried aboard NASA spacecraft. The responsibility for the successful operation of an experiment should be borne largely by the Principal Investigator who is highly motivated for various personal and career reasons to have a productive flight, rather than by an inspector. It would be illuminating to compare total costs and performance of various technically equivalent space plasma experiments flown aboard NASA and Department of Defense vehicles (the two agencies have quite different documentation standards) to understand the origin of any cost differences. Such an examination could yield a number of important lessons concerning the future implementation of NASA's scientific program.

In the long run, it may prove much more economical to decrease reliability for some types of science missions. At present, it typically costs more per kilogram to develop a scientific payload than it does to place that payload in low Earth orbit; however, the ratio can vary from 1:1 to 50:1. A significant fraction of the development cost is associated with the requirement for high technical reliability and quality assurance, as well as by the infrequency of launch opportunities. It would appear that a major reanalysis of the cost/ benefit ratio of reliability requirements on low Earth orbit missions is in order. Again, the experience of other agencies

(e.g., the Department of Defense) or other spacefaring nations may prove instructive in this regard, and a detailed examination of various approaches to program implementation is needed. The possibility of decreasing reliability on certain science missions is especially appropriate as the era of the Space Station approaches, with its potential for the repair and refurbishment of satellites. Reductions in the development and construction costs of major missions that do not have to be designed for reliable long life could have a dramatic effect on the amount of science per dollar produced by the NASA program. However, it is recognized that the cost reductions which may result from the easing of reliability requirements for some science missions might well be reintroduced by the form of extra expenses of making those same missions repairable. Such tradeoffs need to be explicitly examined.

The use of available resources could also be optimized by implementing each project in the way which is most appropriate for that project. Launch vehicles provide an excellent example of the inefficiences that can be introduced by making a poor choice concerning program implementation. Programmatic arguments have been presented earlier in this report for returning to the use of expendable launch vehicles (ELV's). There are also cost arguments which must be considered. In addition to the obvious issue of a direct comparison of the actual costs of using ELV's or the Shuttle for launches, there can also be extra costs associated with the development of a project for flight on a manned rather than an unmanned system. Meeting the exacting safety and other requirements for flight on a manned system inevitably

introduces extra complexity and hence extra cost. Extra work (and hence extra time and cost) is also introduced into the integration process when a vehicle is to be mated with an upper stage and then the whole package integrated into the Shuttle rather than a simpler integration of spacecraft and launch vehicle.

While we have argued that, in some cases, the use of a common spacecraft for a series of missions may be the most cost-effective way of proceeding, in other cases a mission may have unique requirements necessitating a specially designed spacecraft, and the costs of redesigning instrumentation and adapting an existing spacecraft for such a mission may be higher than just proceeding to build a unique spacecraft. It would be a mistake to force everything into a common approach; each case must be examined individually. Arguments concerning the virtues of specifically building spacecraft to be repaired, maintained, and upgaded in orbit in order to have long-lived missions may need a careful reexamination in order to see whether the current conventional wisdom is indeed correct. As the Hubble Space Telescope program has shown, substantial extra costs can be associated with design and development of an on-orbit replaceable spacecraft. Components have to be located where they can be reached, subsystems have to be specially built to be replaced in orbit, and systems have to be designed for astronaut safety. The question can be asked whether it might not have been less expensive to have proceeded with a program involving several copies of a simpler spacecraft, and have long-life of the program (as well as the periodic upgrading of focal plane instruments) achieved through the use of multiple spacecraft

launched sequentially. The answer to this question is far from obvious. It is also far from obvious whether technical, financial, and political considerations would necessarily lead to the same conclusion. With a number of additional major missions designed for both long life and servicability now in planning, a more careful look needs to be taken at the advantages and disadvantages and the economics and the politics of various possible approaches to implementing such programs.

On the basis of the considerations that have just been discussed, we recommend that the Office of Space Science and Applications (OSSA) conduct a fundamental reexamination of its approach toward the implementation of flight projects with the aim of substantially reducing mission costs by the use of similar but appropriately modified spacecraft for several missions, reducing requirements for documentation, reappraising the level of reliability needed for each mission, more realistically matching mission needs and spacecraft and instrument capabilities, and adopting the most appropriate mode for the implementation of each flight program.

The trends described in Chapter 2 are leading, as the result of scientific developments, towards larger, more complex, and longer-lived missions. In spite of this trend, the development (and operations) costs of such missions must be contained. Otherwise the Space and Earth Science Program is in danger of pricing itself out of existence. The issue of taking new approaches to containing program costs must be addressed.

Optimizing Program Implementation

Developing new approaches for reduction of mission costs is only one of several steps necessary to manage resources more effectively. Once a program has been started, its development must proceed on a timely, stable course. As noted in Chapter 2, a significant fraction of the OSSA budget, and of scientists' time, is currently wasted by delays and stretchouts of flight projects. Three recent notable examples. which illustrated the problems introduced by such stretchouts even prior to the Challenger accident-imposed delays, are Spacelab 2, Galileo, and the recently cancelled Solar Optical Telescope (SOT). Spacelab 2 escalated from an initial budget of \$27 million to a final cost at launch, five years later than originally planned, of \$70 million. As the launch date of the Galileo mission slipped from 1982 to 1986, and as the baseline launch system also kept changing, OSSA's costs for this mission rose from \$379 million to \$843 million. This cost will now increase even further due to the additional delay resulting from the Challenger accident and the recent policy decision which has led to the cancellation of Shuttle/Centaur upper stage. Three years of delays in SOT led to an estimated cost increase of \$73 million, a factor which was a significant element in the decision to cancel the program. However, increasing the costs of a program by delaying it and then cancelling it because of those increases does not appear to be a particularly effective way to manage a program. If SOT had proceeded as planned, the effect of delays on these three projects alone would have accounted for \$580 million in increased costs to

OSSA over five years. Were it not for these slips, another new mission could have been developed during this same period without increasing OSSA's level of funding! The effective "loss of funds" from these three missions alone amounts to nearly 10 percent of the yearly OSSA budget and is equivalent to one-third of the annual Research and Analysis budget. These problems have, of course, now been drastically exacerbated by the substantial stretchouts which have resulted from the Challenger accident. Such delays are extremely harmful to the vitality of space research. They produce no useful science and represent a diversion of resources that might otherwise allow additional projects to be carried out and provide new flight or other research opportunities for the Space and Earth Science community. Once a project has been started, it must be completed on the most cost effective schedule. The fact that there is a most cost effective schedule must be recognized not only by NASA but by the Office of Management and Budget and by Congress as well. Recent Congressional actions whereby changes in funding requests have been made or funding limitations have been imposed have forced significant changes to program schedules and have been a contributor to the current problem.

Other significant steps can also be taken to help minimize this effective loss of resources and more effectively manage the OSSA program.

(a) Flight projects should not depend upon the success of other major concurrent technology development efforts. In particular, a flight project should not be started until the

launch or carrier vehicle is assured and a clear understanding exists of the risk associated with any necessary new technology connected with that carrier. Galileo is perhaps the prime example of what can go wrong in this area, with numerous launch slips associated with delays in the Shuttle availability, with lack of capability of the Inertial Upper Stage, followed by development of a kick stage which was then not used, followed by development of a Centaur to be used with the Shuttle, followed by a policy statement disallowing that use on safety grounds and cancelling the Shuttle/Centaur development. Although there are a number of plausible options, at the present time, it is not obvious how Galileo is going to be launched. These mistakes must not be repeated. It would be premature, for example, to initiate the development of major flight missions which require large ion-propulsion systems or on-orbit refurbishment and assembly at the Space Station until those capabilities are understood and assured. In addition, analogous problems resulting from the selection of Spacelab experiments prior to the Shuttle development schedule being well understood, and the overruns which resulted from the subsequent delays in Shuttle/Spacelab must not be repeated during the development of Space Station laboratories and facilities.

(b) New approaches to managing and planning the OSSA program should be considered. A possible approach to more efficient and cost effective planning of moderate scale missions can be found in the Explorer program where missions are developed and launched essentially one or two at a time within a fixed funding

envelope, with a new mission not being started until its predecessor has passed the peak of its spending curve. At the very least, this approach has the virtue of bounding any funding problems with individual missions within the fixed envelope without impacting the rest of the OSSA program. The reward for effective management of one mission is the ability to start the next one, a type of incentive which might have wider application. Care must also be taken not to commit such a level of effort program for too long a period of time. Some Explorer missions now under development, and which will not be launched until the late 1980's or even 1990's, were selected in the late 1970's. A recent report of the National Academy of Sciences (Committee on Solar and Space Physics Explorer Report) suggests that additions to the Explorer queue be made a few at a time, every few years. In fact, NASA now has released a "Dear Colleague" letter soliciting new ideas for Explorer missions which proposes to follow this course for adding new missions. Other modest scale programs (such as the Planetary Observers) should be effectively funded in a similar fashion and would provide the ongoing flight opportunities we have argued are so important. As is evident from the funding curves in Figures 1 and 4, the size of the funding peaks and valleys introduced by the large missions are such that they obviously cannot be handled in such disciplineunique level of effort programs but must be considered in the broader context of planning the overall OSSA program; they must also be subjected to the ordered decision-making process described in Chapter 6.

(c) Total run-out costs, including operations and data analysis costs, should be well understood and recognized before a project is officially started. In many cases cost problems have arisen from an overlyoptimistic estimation of the rapidity and technical ease of completing flight projects. Large cost overruns in major projects can severely damage the entire Space and Earth Science Program. Such overruns affect not only the specific project but also every other element of the OSSA program. They call into question OSSA's ability to manage such projects and undermine the prospects for undertaking new missions. They can no longer be tolerated.

The present system of nonadvocate reviews, provided they are rigorously conducted, is a significant step in the right direction. Such careful scrutiny prior to project start is strongly encouraged. These nonadvocate reviews were a direct result of a study on NASA project management which was carried out several years ago under the direction of Donald P. Hearth, the former Director of the NASA Langley Research Center. With the large turnover in NASA management which has taken place since the completion of that study, it would, perhaps, be worthwhile to revisit its conclusions to ensure that proper attention is being given to preproject definition and technology efforts.

(d) When a major delay or descoping of a program appears inevitable, OSSA should directly address the questions of whether the program is still viable and whether it continues to be scientifically competitive with ongoing or proposed

OSSA programs in all fields. It should not be automatically assumed that any program once started should be continued no matter what the circumstances. The scientific integrity of a descoped or delayed mission may be so seriously impaired that the basis on which the program was selected in the first place is no longer valid. Delays can be as, or even more, debilitating as descoping for several reasons. As noted earlier, they can generate significantly increased costs and preclude other programs; long-delayed data may no longer address forefront scientific issues; the program may no longer be able to contribute to the original goals of a coordinated international effort; or the return expected from the program may simply no longer be worth the price.

(e) A more conservative policy should be adopted towards initiating Phase B studies and promoting New Starts, one which recognizes that the limited resources available should be concentrated on the definition of a relatively small number of projects, and that future opportunities for the start of large-scale programs may be restricted. Money should not be wasted in studies of large numbers of missions which do not have a reasonable chance of getting started or progressing to launch.

Towards More Realistic Program Planning

In order to optimize the use of resources, OSSA must take a more realistic look than it has in the past at the prospects for new programs, and then adjust its activities accordingly. There should be an orderly, well-planned evolution for major missions. The level of preproject support for instrument

development and the optimum timing of the release of the Announcement of Opportunity (AO) for flight experiments must be carefully considered. Some new developments may require experimental testing on the ground or in suborbital missions before they are ready for inclusion on major missions. In other cases, such as in Earth-directed radar or imaging observations, short duration orbital flights may be needed to properly refine the design of instrumentation before deployment on a major long-lifetime mission. Only after appropriate preliminary steps have been taken should the Announcement of Opportunity for flight experiments be released and the flight project started.

The timing of the release of an Announcement of Opportunity is a particular issue requiring further consideration. The effort expended in writing competitive proposals is appreciable, as are the time and energy spent participating in mission studies and other related definition and technology development activities. Time (and money) are diverted from other possible scientifically productive activities. Announcements of Opportunity should only be released for those projects that have some reasonable prospect of being funded and going into development within a few years following investigator selection. There have been too many examples of projects being cancelled following the release of an AO and receipt of proposals, or projects that have entered years of definition without an actual start being in sight. The resulting wasted effort and unfulfilled expectations squander scientific energy and enthusiasm and divert limited resources which might be better used in other ways. At the very

least, AO's and related briefings should be frank about selection and program prospects so that aspiring Principal Investigators can realistically estimate their chances for success before the preparation of proposals. The AO process should be used to select the best ideas and experimental approaches. It must not raise false expectations. A similar statement can be made concerning community involvement in mission definition activities.

A realistic limitation must be imposed on the number of projects for which Phase B mission definition studies are undertaken. At present, the number of candidate missions for which Phase B studies are in progress or proposed is far greater than the number of flight project New Starts that can be expected within any reasonable period of time. Since Phase B studies involve expenditures of appreciable resources, and since they imply a commitment that those projects are likely to progress to flight project status, a formal procedure utilizing the criteria presented in Chapter 6 should be applied not only to the programs to be started but also to limit the number of candidates for mission definition. Such studies should be carried out only for those projects which are of the highest merit and which are truly serious candidates to be taken into development. In addition to not using resources in an optimum fashion, an overabundance of Phase B studies can also mislead the scientific community concerning the prospects for the start of a given mission and become a hindrance rather than a help to orderly and realistic planning.

In recognition of the growing space capabilities in countries other than the U.S., it is also important that NASA foster a well-coordinated planning process with our non-U.S. colleagues. Many missions now underway or under discussion are, in fact, international efforts. In Chapter 2, we discussed the emergence of the strong capabilities in other nations. In Chapter 5, we have recommended a continuation and expansion of efforts toward establishing reciprocity of flight opportunities. If such efforts are to be successful, the U.S. must be perceived as a dependable partner. There also must be a strong effort placed on international program coordination and planning. Our past collaborative successes are numerous (see Logsdon, 1984: Rosendhal, 1986). Research scientists both in the U.S. and abroad have similar, if not identical, goals and have worked well together. However, a few specific disappointments due to mission delays or cancellations can damage prospects for future joint research missions. When decisions involve joint commitments between the U.S. and other nations it is imperative that efforts be well coordinated during the planning phase and that all the partners be fully informed if delays or problems occur on either side. The difficulties of meshing diverse planning, budgeting, approval, and experiment selection processes and cycles must be recognized and allowed for in any coordinated planning process. Doing this will require a substantial effort. However, recent successes in the planning of new international initiatives (e.g., U.S. participation on the European Space Agency SOHO and Cluster Missions and the Japanese Geotail and High Energy Solar Physics missions) show that it can be done. The potential benefits to all partners of such careful planning and coordination are also substantial, and in the long run will be well worth the effort required.

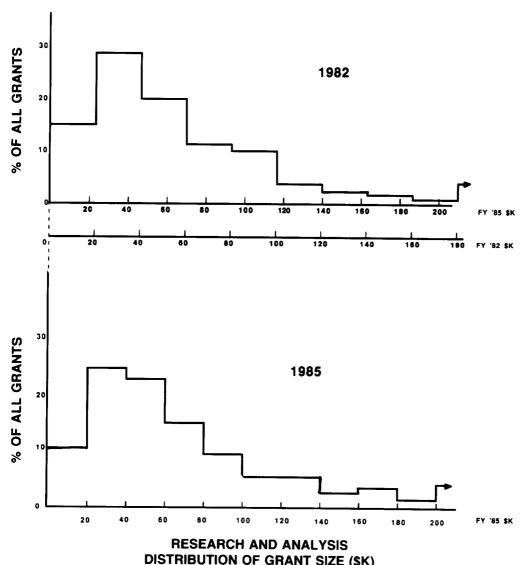
Optimizing the Use of Research Funding

Attention thus far in this Chapter has been focussed on optimizing the use of resources for carrying out flight projects. However, we have also argued in Chapter 4 that the basic research activities are as important as the flight projects and use of these valuable resources must be optimized as well. A variety of approaches can be suggested, not any of which alone may solve the funding optimization problem, but which taken together and appropriately applied could make major improvements in the use of time, talent, and funds. These approaches include use of larger and perhaps multiyear grants, use of consortia or teams for certain kinds of research programs, and more innovative use of guest investigator programs.

Generally speaking, research grants are awarded primarily to individual researchers who may support graduate students or postdoctoral associates and then comprise a small team. The critical mass of research talent and laboratory, library, and computational facilities needed to facilitate excellent research often means that various institutions (university departments, national centers, and so on) specialize in certain disciplines, but then join in the formation of broader discipline-oriented research groups.

In order to examine certain aspects of the allocation of research funding, OSSA, at SESAC's request, compiled data displaying the distribution of funding levels of research grants in six science disciplines. Information on the number and size of research grants is shown in Figure 8 which is a histogram of the

distribution of grant sizes for the six disciplines for FY 1982 and 1985. There are, of course, lengthy histories in each science discipline that have led to how grants are used and why grants are the size they are, and the reasons which have led to the current situation need to be carefully understood in each case. The total number of grants awarded in these disciplines is seen to increase by 16 percent between 1982 and 1985 while the average funding level in real year dollars per grant decreased in spending power by 8 percent. In order to see what these figures mean, a crude estimate of the costs of university research in the mid-1980's is useful. Although these estimates are highly discipline dependent, about \$40,000 per year is needed to support a typical faculty member if only summer salary plus funds to cover some computer, secretarial, travel, and publication expenses are awarded; costs of any necessary experimental equipment must be added to this. The salary of a young postdoctoral associate, once fringe benefits and overhead are added, is more than \$50,000 per year; a senior scientist who is funded only by grants requires somewhere around \$100,000 per year. At a typical private university a graduate student's tuition plus living expenses would be close to \$20,000 per year. It is evident from the data that, in several of the disciplines examined, both the average and the most probable grant sizes are significantly less than the amount which is necessary to support the research expenditures of a faculty member who also furnishes funding for any associated students or postdoctoral researchers. Thus, many scientists who try to sustain even a small research group require several different grants just to cover a single



DISTRIBUTION OF GRANT SIZE (\$K)

AVERAGES

DISCIPLINE	FY 82\$ ACTUAL	FY82\$ INFLATED TO FY85\$	FY85\$ ACTUAL	MOST PROBABLE RANGE
ASTRONOMY/RELATIVITY	63	75	85	20-40
PLANETARY ASTRONOMY	80	96	90	20-40
PLANETARY ATMOSPHERES	36	43	47	20-40
SPACE PLASMA PHYSICS	79	95	83	20-40
UPPER ATMOSPHERE	161	193	174	60-80
OCEANS	103	123	93	40-60
SIX DISCIPLINE AVERAGES	87	104	96	20-40

Figure 8. Distribution and Sizes of Research Grants.

This figure shows two histograms of the distribution of grant sizes in dollars plotted as a percentage of all grants for the years FY 1982 and FY 1985. Six scientific disciplines of astronomy/ relativity, planetary astronomy, planetary atmospheres, space plasma physics, upper atmosphere, and oceans research have been included in the data. The actual average grant size in dollars is indicated in the accompanying table for each of these disciplines. Notice that the most probable range for a grant is \$20K to \$40K for four of the six disciplines, and this leads to the peak in the histogram distribution for both FY 1982 and FY 1985. The upper atmosphere and oceans research averages are somewhat above this range but also have not changed between FY 1982 and FY 1985. The average grant size is larger than the most probable range in all cases due to the presence of a few rather large grants in each discipline which show the distributions.

year's effort. The preparation of several successful proposals each year compels scientists to expend an inordinately large amount of time in writing (and reviewing) proposals as well as negotiating grants. In order to increase scientific productivity, the time spent in proposal writing and negotiation must be reduced wherever possible.

An apparently obvious and simple solution to the problem of grant size would be to increase grant amounts by combining related tasks or research efforts at a single university, and to encourage proposals for larger, more comprehensive, multiyear efforts whenever appropriate. However, there are complexities to this issue that also must be considered. For example, if only large grants were funded, a likely outcome would be that established researchers would be preferentially supported and younger scientists just beginning their research careers might be placed at a disadvantage in the competition for funds. The receipt of grants is so important for the professional advancement of young researchers that care must be taken in attempting to solve one problem not to create a worse one which might have the effect of preventing talented new researchers from being able to establish their own research programs and groups.

Another consequence of the use of small grants is that it may promote narrowly focussed research projects even as it distributes funds broadly. Many guest investigator programs, such as those currently available for the International Ultraviolet Explorer, the Infrared Astronomical Satellite, the Pioneer Venus Program, and the Dynamics Explorer emphasize small grants of short duration. While this approach may be appropriate for some cases and

has the salubrious effect of involving a broad cross-section of scientists in these programs, the important tasks of combining ground data with space data and of combining information from separate missions to study broad problems may be prevented by the administrative structure. Furthermore, this strategy tends to deemphasize what should be a long-term commitment to addressing the important underlying physical questions raised by the exciting data which emerge from the missions. In this case some steps are, in fact, being taken to deal with this problem. To promote such use of complementary data sets, for example, OSSA is about to release a "Dear Colleague" letter announcing continuing research opportunities in the Space Astrophysics Data Analysis Program wherein use of data from several space astrophysics missions to study broad problems involving multispectral data sets can be covered in only one proposal. Other efforts of this type should be strongly encouraged.

With the growth of broad research questions and extensive data bases, as well as the ability to link researchers at diverse locations through computer networking, some research problems may be better addressed by interdisciplinary, multiinstitutional, collaborative research teams than by single individuals. In some sense this is analogous to the large spacecraft flight teams which contain specialists from several research fields who are needed to develop complex instruments. Important steps toward the implementation of such a team approach have been taken in the Astrophysics and the Solar-Terrestrial Theory programs as well as in the program for interdisciplinary research in the Earth Sciences. In these cases, NASA has deliberately encouraged the forma-

tion of groups of critical size, usually including postdoctoral researchers and graduate students, to address broad questions. Annual funding for many of these groups is in excess of \$200,000 and support has been provided for extended periods of time. A significant fraction of these efforts also involve the participation of individuals from several institutions. Results to date from such programs have shown that this approach can be extraordinarily effective. The team approach may also be effective in other areas such as research programs involving the development of instruments or technology, especially if the work is such that it must involve collaborations between industry, universities, and NASA Centers.

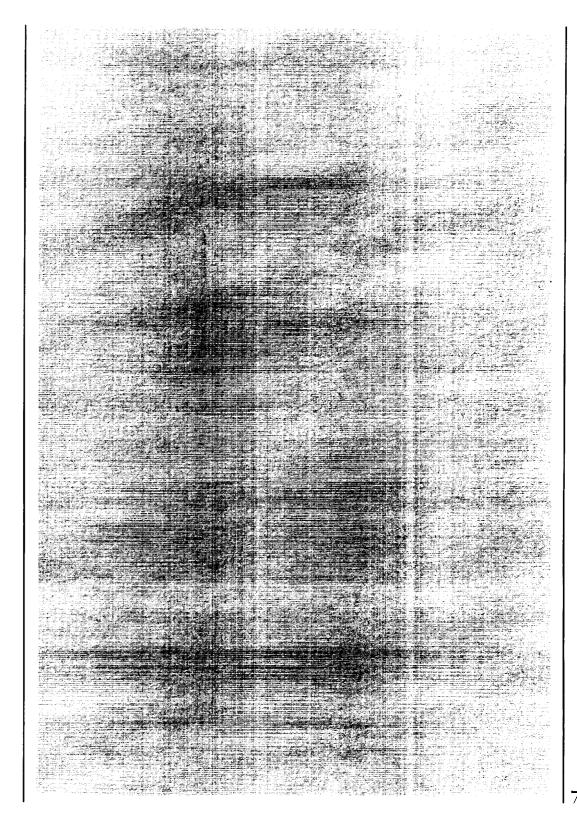
The formation of consortia is not the answer to all problems and must not discourage the individual investigator and innovative "small science." Just as in the case of flight opportunities, the appropriate spread of research activities occurs across a spectrum of sizes from the individual investigation, to the small group, to the large research team approach. As is true with so many aspects of the space program, a rational balance is necessary.

The Need for Margins in NASA's Program Planning

Perpetually tight budgets have driven NASA to a situation in which a single mishap can devastate its entire program. Thus the failure of an O-ring has grounded NASA's total launch fleet and brought experimental Space and Earth Science to an abrupt halt. Much of the damage could have been avoided by maintaining a mixed fleet of manned and unmanned launch vehicles. We are concerned that there may be other

possible single-point failures in the NASA system that also have the potential for severely damaging the progress of space research. Failure of the Tracking and Data Relay Satellites or their ground station, for example, could lead to simultaneous loss of all the data from the Hubble Space Telescope, the Gamma Ray Observatory, the Upper Atmospheric Research Satellite, and other missions as well. All such possibilities for single point failures must be examined and plans for contingencies and alternatives must be developed.

The evolution of the Space and Earth Science Program toward the use of large, facility-class missions not only centers the major direction of each discipline for a decade or more around such facilities, but also increases the possibility for devastation that can be wreaked upon an entire discipline by the loss of a single mission. At the same time, the potential for disaster has been increased by decisions to build and launch only a single spacecraft for each mission-even for irretrievable and unrepairable spacecraft such as Galileo. Even though projects may have strong programs for reliability and quality control, and spacecraft may have many redundant subsystems, it is unrealistic to expect space missions to be 100% successful. In fact, we have previously pointed out the possible advantages of decreasing the documentation and quality assurance requirements for some types of missions. Therefore, NASA planning must not be so success-oriented that unforeseen mishaps can decimate large elements of its scientific program. NASA seems to be planning its program so that there is no longer any margin for error. The wisdom of such tight planning needs to be carefully reconsidered.





Chapter 8:

A Time for a New Commitment

A Proud Beginning

The modern era of space research had its beginnings in the years following World War II. Captured V-2 rockets launched from White Sands, New Mexico, carried a variety of scientific instrumentation to altitudes far above the Earth's surface. These investigations studied ultraviolet radiation and X-rays from the Sun, as well as the constituents of the Earth's ionosphere, and what came to be known as the radiation belts and the magnetosphere—discoveries that led to a deeper understanding of the interaction of Sun and Earth.

From these studies of the Earth and Sun, space research expanded to investigations of the solar system, and the universe beyond. X-ray investigations of the Sun soon led to X-ray and gamma ray observations of distant stars and galaxies. Satellites and spacecraft leaving the confines of the Earth's gravitational field, in the early 1960's, led to studies of the Earth and exploration of the Moon, interplanetary space, and the nearby planets.

By the early 1980's, astronomical studies in virtually all wavelength regimes had become possible, spacecraft had flown past all the planets known to the ancients, primary cosmic radiation from space was being studied, Earth resources were being monitored, the oceans and the atmosphere were being surveyed, sophisticated missions to test theories of gravity were under development, and the manned Space Shuttle was coming into use as the United States' sole transportation sys-

tem into space—replacing the expendable rockets of earlier decades.

Throughout these years, the Space and Earth Sciences established themselves as unique. The challenging observations they undertook stimulated important technological advances. Added to this was the air of drama drawn from the excitement and risk associated, particularly in the later years, with manned space flight. And there was a fascination for the American people with images and data obtained from spacecraft yielding insight into the nature of our planet, our solar system, and the greater universe far beyond.

The unique power of the Space and Earth Sciences derive from the advantageous observations and measurements made by leaving the Earth's surface. Studies of planets, stars, and galaxies with instruments aboard Earthorbiting satellites avoided the interference of the atmosphere and achieved improved accuracy, resolution, and above all, wavelength coverage. Missions to planets, moons, and comets garnered precision and breadth of information available only from close scrutiny or surface sampling. The complex space environment—plasma clouds and magnetic and electric fields—could only be analyzed and understood by immersing space probes within those plasmas. Observations of the Earth from space offered a unique perspective: the possibility of studying surface properties and processes identifiable only from space, and the potential of long-term observations intended to detect and document change on a global scale. And, finally, the life sciences and

materials research communities began utilizing the microgravity environment of space.

Midcourse Doldrums

The sheer audacity and breadth of this program carried its own excitement. But it also required dedicated support from the American public. The foregoing chapters have documented the fact that by the mid-1980's there had developed serious stresses, as a result of which the expectations and needs of a vital space research community had not been able to be fulfilled. Program delays led to cost overruns, and there were even cancellations of missions well advanced towards launch. Programs involving international agreements were also not imune to cancellations, with serious long-term consequences. The system was losing momentum.

Serious as these problems were, they failed to alert the Agency or the science community to the fundamental issue concerning breadth of the program.

How Broad a Program?

NASA at present is attempting to carry out an increasingly broad Space and Earth Science Program. The Office of Space Science and Applications supports diverse scientific disciplines that are concerned with questions ranging from the nature of the core of the Earth to the origin of the Universe. The Office also supports programs in the life sciences, materials research, and communications research, areas not included within the Space and Earth Sciences. Ambitious, often costly proposals for future flight projects have been devel-

oped by the various science and technology disciplines. The scientific community has been encouraged to develop such plans to take advantage of space opportunities. Both the scope of this program, and the current stresses to which it is subject, clearly raise the question of whether NASA will actually be able to support such a wide ranging program during the next few decades. The attempt to proceed with a very broad program has already led to a virtual log jam of new missions, a situation that contributes significantly to the present distress of the scientific community.

While some progress can be made through management improvements, a deeper question remains concerning the ultimate course of the Space and Earth Science Program. There are two alternative paths that could be taken. and it is time for a conscious decision to be made concerning which one to follow. Each would have a profound, but different, long-term effect on the scientific community and on the nature of the NASA Space and Earth Science Program. The first alternative requires a decision to provide adequate resources to carry out a comprehensive program. NASA would then explicitly commit itself to the support of a full range of scientific studies crucial to the advance of the Space and Earth Sciences across a broad front. While some of the requisite funds could come from more efficient management of resources, as outlined in Chapter 7, from collaborative arrangements with other Federal agencies, or through international collaboration, at the present time there still remains a clear mismatch between possibilities and prospects.

If adequate funds are not to be provided, the second alternative requires

that the scope of the scientific program be reduced to fit available present and projected resources. A much more specialized program in the Space and Earth Sciences would then be inevitable, and OSSA would be forced to terminate its research activities in selected fields. Arriving at decisions as to which scientific disciplines would be pursued and which would be dropped would be very painful and would inevitably produce severe dislocations in the scientific community.

Clearly, the decision between these alternative paths cannot and should not be made by NASA or the scientific community alone. It also should not happen by accident. It is a national decision requiring a concensus of the American people, and thus of their representatives in the Executive and Legislative Branches of Government. Achieving such a concensus will be an important turning point for American science.

Steps Towards a Promising Future

A scientific discipline is kept alive and vigorous when stimulating questions can be posed and means are at hand for providing clear answers. Space and Earth Science is replete with provocative questions, imaginative theorists in the space community have developed innovative ways for constructing predictive models based on experimental and observational evidence, and a talented community of instrumentalists knows how a next generation of space missions capable of testing these models should be designed. But the opportunity for building these space missions to sustain a continued data flow has been seriously declining over the past few years and the base support for innovative research and analysis has also suffered. Particularly in the wake of the Challenger accident, the future looks bleak for established space researchers and uninviting to the talented young researchers whom a productive field must continue to attract to remain vigorous and enterprising. If the most gifted researchers are to remain in the field, if Space and Earth Science projects are to stay alive and healthy, then the various disciplines must display a promising future. That future can be assured by establishing a number of favorable conditions to foster excellence.

First, we believe that the range of space activities we undertake must be kept broad. Each subdiscipline of Space and Earth Science learns from advances made in related areas. Small and large undertakings should be interwoven in ways best suited to progress in each given field. Manned interaction should complement automated instrumentation, and launch vehicles should be chosen to meet technical demands. The launch vehicle fleet must, therefore, use both manned and unmanned vehicles. There are urgent requirements for both expendable launch vehicles and for the manned Shuttle. Steps to restore the vitality of the flight program, however, will not suffice unless healthy support for basic research and data analysis on the ground complements activities in space; research and analysis meld isolated observations into coherent models which form the basis of new scientific understanding. The expense of space missions can only be justified if they are part of a coherent scientific program and produce major new insights into nature. These major programs will often yield the greatest gains only if complemented by smaller missions. This necessitates a clearheaded appraisal of the most appropriate mix of mission types needed to carry out the most effective overall space research effort.

Second, a promising future can only be assured through the existence of a sound infrastructure. The overall value and uniqueness of each of the contributing classes of institutions, whether government, industry, or university establishments, must be recognized if a diverse, productive Space and Earth Science effort is to be sustained.

Third, the means by which we decide on the direction of future research for years to come must be based on a systematic framework for evaluation of competing proposals. Major missions that have the potential for providing vast leaps in understanding or promise substantial benefits for society will only continue to remain at the center of attention as long as the perceived advances warrant appropriation of the required funds. In Chapter 6 we have presented a set of criteria that may be used to set priorities and decide among major missions and initiatives promoted by the scientific community and competing for status as funded new projects intended for launch into space.

We must be clear, however, that these recommendations can only succeed if NASA and the science and engineering communities take steps to improve the effectiveness with which Space and Earth Science programs are managed and resources are utilized. Mission costs must be reduced and there are clearcut ways to reduce them. Once a project has started it must be completed on the most cost-effective schedule. Considerable cost saving appear possible in OSSA if available resources are optimally used, and steps should be taken to promote this, in addition to seeking increased funding for space

research.

The Need for A Steadfast Course

High funding levels alone are not the sole answer to budgetary problems. Equally significant is budget reliability. Orderly and effective conduct of a Space and Earth Science project and cost effective execution can only be planned if future budgets can be predicted. Hassler's principles are as vital today as they were in 1807. Steadiness in financial planning is absolutely essential to the process. This becomes doubly true when projects are carried out collaboratively with other countries. And though technological difficulties can lead to unanticipated delays and expenditures, effective means can often be found to reallocate manpower and resources to minimize the financial impact of setbacks. That is what effective management is all about. However, without steadfast planning there can be no clearly perceived future; and with an uncertain future, the talent will not be attracted into the Space and Earth Sciences.

The New Commitment

Recent strategy reports by the National Commission on Space on "Pioneering the Space Frontier" and by the National Academy of Sciences on "Major Directions for Space Research, 1995-2015" have stressed the need for imaginative long-term thinking and an expanded future for space projects. The National Commission envisages exploring, prospecting, and settling the solar system in the years ahead. The Academy's report foretells the construction of enormously powerful structures for Space and Earth Science research, assembled and refurbished by astronauts in space.

Even prior to the Challenger accident, the Space and Earth Science Program had been under stress. Now it has suffered a profound setback. We must get the endeavor going again. We must recover the strength this effort once could proudly claim. We must turn to the Executive and Legislative

Branches of the nation's government to solicit the support we need for conducting a program of which the American people can be proud—achieving advances in our understanding of the Earth, the solar system, and the universe that will contribute to the enlightenment of man and the future of mankind.

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Appendix

This study of the Space and Earth Science Program of NASA began in October 1984. During the first phase, which lasted from October 1984 to June 1985, a significant fraction of the meetings of the Space and Earth Science Advisory Committee were devoted to wide-ranging discussions of the issues in order to formulate a set of questions to be addressed in a study report. During the second phase, which lasted from October 1985 to October 1986, working groups were formed to address specific questions intensively, the full Committee considered and debated reports from these working groups at its regular meetings, and the final report was prepared. Throughout this entire interval a subset of the full Committee met regularly as first a Planning Committee and then a Writing Committee to organize and guide the study, synthesize reports from the Working Groups and the discussions of the full Committee, and prepare the final report. Meetings of this small group, ranging from 1-3 days in length, were held in December 1984 (Johns Hopkins University), April 1985 (Space Telescope Science Institute), October 1985 (Johns Hopkins University), April 1986 (Pennsylvania State University), August 1986 (Cornell University), and September 1986 (NASA Headquarters). The membership of the full Committee which participated in one or both phases of this study is listed in this Appendix as is the membership of the Planning and Writing Committees. Report drafts were submitted to the membership of the full Committee in May 1986 (for discussions at the June 1986 meeting) and September 1986 and all comments received from Committee members were considered in subsequent drafts. The final version was prepared on behalf of the full Committee in October 1986 by Louis J. Lanzerotti, Chairman of SESAC.

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Acronym List

AMPTE Active Magnetospheric Particle Tracer Explorer

AO Announcement of Opportunity
ASC Astronomy Survey Committee
ASO Advanced Solar Observatory

AXAF Advanced X-Ray Astrophysics Facility
CRAF Comet Rendezvous/Asteroid Flyby

CRRES Combined Release and Radiation Effects Satellite

CSSP Committee on Solar and Space Physics

ELV Expendable Launch Vehicle EOS Earth Observing System

ERBE Earth Radiation Budget Experiment

ESA European Space Agency

ESSC Earth System Science Committee

GAS Getaway Special

GGS Global Geospace Science

GP-B Gravity Probe B

GRM Geopotential Research Mission GRO Gamma Ray Observatory

HEAO High Enegry Astronomy Observatory HRSO High Resolution Solar Observatory

HST Hubble Space Telescope

ISAS Intitute of Space and Astronautical Science (Japan)

ISEE International Sun-Earth Explorers ISPM International Solar Polar Mission

ISTP International Solar-Terrestrial Physics Program

LDR Large Deployable Reflector MAGSAT Magnetic Field Satellite

MO & DA Mission Operations and Data Analysis

MSR Mars Sample Return
NAC NASA Advisory Council
NAS National Academy of Sciences
NCS National Commission on Space

NOAA National Oceanic and Atmospheric Administration

NRC National Research Council
NSCAT NASA Scatterometer
NSF National Science Foundation
OMB Office of Management and Budget

OPEN Origin of Plasmas in the Earth's Neighborhood OSSA Office of Space Science and Applications OSTP Office of Science and Technology Policy OVLBI Orbiting Very Long Baseline Interferometer SESAC Space and Earth Science Advisory Committee

SIRTF Space Infrared Telescope Facility

SMM Solar Maximum Mission

SOHO Solar and Heliospheric Observatory

SOT Solar Optical Telescope

SS Space Station

SSB Space Science Board

SSEC Solar System Exploration Committee STO Solar-Terrestrial Observatory

TFSUSS Task Force on Scientific Uses of the Space Station

TFTR Tokomak Fusion Test Reactor
TOPEX Ocean Topography Experiement
TSS Tethered Satellite System

UARS Upper Atmospheric Research Satellite

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